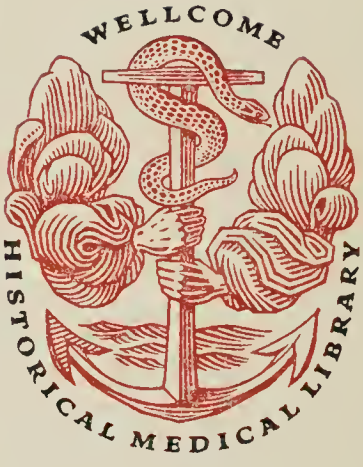

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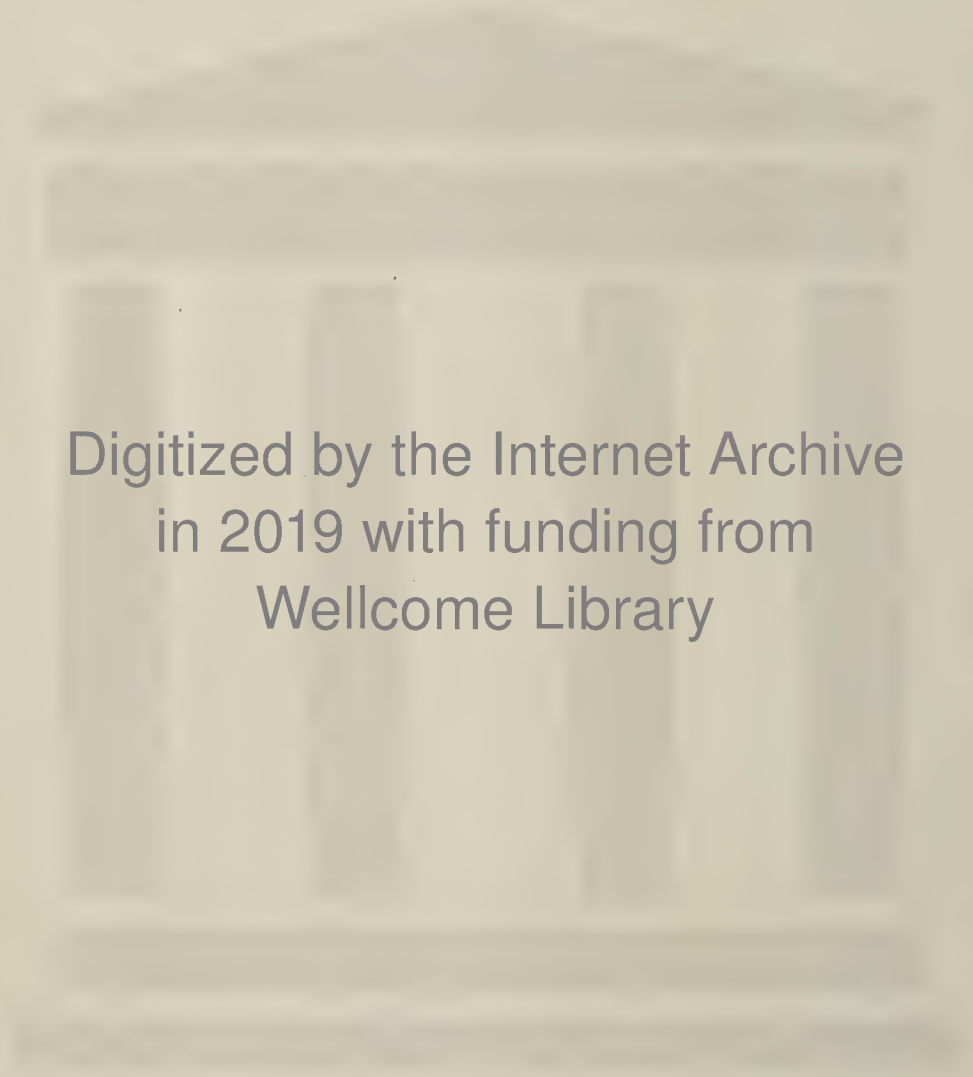
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THE PHILOSOPHICAL

HISTORY

AND

MEMOIRS

OF THE

Royal Academy of Sciences at *Paris*:

OR,

An ABRIDGMENT of all the PAPERS relating to *Natural Philoſophy*, which have been publiſh'd by the *Members* of that *Illuſtrious Society*.

Illuſtrated with COPPER-PLATES.

The Whole Tranſlated and Abridged,
By *JOHN MARTYN*, F.R.S.
Profeſſor of Botany in the Univerſity of *Cambridge*;
AND
EPHRAIM CHAMBERS, F.R.S.
Author of the Univerſal Dictionary of Arts and Sciences.

VOL. I.

LONDON:

Printed for JOHN and PAUL KNAPTON, in *Ludgate-ſtreet*; and FRANCIS COGAN, and JOHN NOURSE, near *Temple-Bar*. M.DCC.XLII.

Gale. 3
Eng 170

To the READER.

THE Bookſellers having been informed, ſince the Publication of this Abridgment, that the late Mr. *Chambers*, Author of the *Cyclopædia*, or *Univerſal Dictionary of Arts and Sciences*, had made ſome Progreſs in the ſame Deſign, were deſirous to make uſe of the Papers left by an Author ſo well eſteemed by the Publick. In the Courſe therefore of this Work, ſuch Papers of Mr. *Chambers* will be inſerted, as come within the preſent Deſign. But as I am unwilling to aſſume to myſelf the Labours or Merit of another, Care will be taken to diſtinguiſh each Paper, which was written by Mr. *Chambers*: which I believe will be acceptable tothe Reader; as he will be able by this means to know, which Parts were compoſed by that ingenious Author, and which by

Chelſea, Nov. 30, 1741. *John Martyn.*

THE PREFACE.

THE knowledge of nature affords ſo great a delight to the mind of man, and is of ſo much importance to the well being of ſociety; that there is hardly any conſiderable ſtate in *Europe*, where it has not been thought neceſſary to aſſemble a body of learned men, whoſe chief employment it ſhould be to improve *natural hiſtory* and *philoſophy*. The ROYAL SOCIETY of *London* was founded for this purpoſe, about fourſcore years ago, by *King Charles the Second*: and what improvements have been made by that learned body, not only with relation to ſpeculative knowledge, but even to the common uſes of life, may be ſeen in thoſe excellent papers, publiſhed from time to time

under the title of *Philoſophical Tranſactions*. Soon after the foundation of the *Royal Society*, the late *King of France*, who was unwilling to omit any thing which might contribute to his glory, aſſembled a conſiderable number of the moſt able men in his kingdom, to whom he gave the title of the ROYAL ACADEMY OF SCIENCES; and engaged them in the purſuit of *natural* and *mathematical knowledge*. But this learned aſſembly was not eſtabliſhed, as a *regular ſociety*, till that monarch, in the beginning of the year 1699, thought fit to give them a new eſtabliſhment, and to regulate their conduct by a body of laws framed for that purpoſe.

From the time of this new eſtabliſhment of the *Royal Academy of Sciences*, a volume of their *diſcoveries* and *obſervations* has been publiſhed every year, under the title of *A Hiſtory of the Royal Academy of Sciences*,

Sciences, by their perpetual ſecretary, the celebrated M. *Fontenelle*.

This *Hiſtory* conſiſts of two parts: the firſt is what is particularly called *the Hiſtory*. In this part, M. *Fontenelle* gives a ſhort account of every remarkable tranſaction, either in writing or by word of mouth, that has paſſed in the academy. The other part contains the *Memoirs*, or pieces that have been read in the *academy*, ſuch as have been judged to be of the greateſt importance, and the moſt worthy of being communicated to the publick at full length.

The papers contained in this curious *Hiſtory* are ranged under ſeveral heads: the firſt of which comprehends thoſe which relate to *natural philoſophy in general*. Under this head the reader will find many remarkable accounts of the nature and properties of the *air*, obſervations on the *thermometer*, *barometer*, &c. diſcourſes on the cauſes and effects of *winds*, on the flux and reflux

of the *ſea*, relations of the effects of thunder, lightning, &c. conſiderations on the *aurora borealis*, and other remarkable appearances in the heavens; curious diſcoveries of the various and ſurpriſing properties of the *loadſtone*; inquiries into the nature of the *magnetical needle*; and many other entertaining as well as inſtructing particulars, which we ſhall not here detain him with enumerating.

To theſe papers on *natural philoſophy in general*, we have added many others, which we thought would be eſteemed to be moſt uſeful and entertaining to the generality of readers. Theſe contain the natural hiſtory and anatomy of *animals*, many uſeful diſcoveries relating to *geography* and *navigation*, a great variety of *optical*, *dioptrical*, and *mechanical* diſcoveries and obſervations, with many other mathematical papers, which relate to common uſes of life, paſſing over thoſe, which are merely ſpeculative, or of leſs general uſe.

The

The *Abridgment* of the *Philoſophical Tranſactions* has been ſo well received by the publick, that we flatter ourſelves the preſent work, which is ſo nearly allied to it, will meet with a no leſs favourable reception. It is an *Abridgment* of all the *Memoirs of the Royal Academy of Sciences*, which relate to *natural philoſophy in general*, to the natural hiſtory of animals, and to the practical parts of the mathematicks; to which we have prefixed ſuch accounts in M. *Fontenelle*'s *Hiſtory*, as are not to be found in the *Memoirs* themſelves.

As for thoſe accounts in the *Hiſtory*, which are found more at large in the *Memoirs*, we have thought proper wholly to omit them; becauſe they are generally no more than a hiſtorical account of what is contained in the *Memoirs* themſelves. And as we have contracted each of thoſe *Memoirs*, into as narrow a compaſs as we could, without injuring

ing their ſenſe, we thought it would be unneceſſary to add the hiſtorical account of them too, which would ſwell the bulk of theſe volumes too much; it being our deſign to lay before the reader the *philoſophical diſcoveries* and *obſervations* of the *Royal Academy of Sciences*, with relation to the particulars above-mentioned, in as conciſe a manner as we could, without diminiſhing any part which really conduces to that end.

If the preſent undertaking meets with the ſucceſs which we hope for, it will be an encouragement to us to proceed in abridging the other papers, which, tho' they are of leſs general uſe than theſe now publiſhed, will, however, be of the greateſt ſervice to ſuch as are engaged in thoſe particular ſtudies.

A

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AN

AN

ABRIDGMENT

OF THE

Philoſophical Diſcoveries

AND

OBSERVATIONS

IN THE

HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris* for the Year 1699.

I. *Compariſons of the obſervations made in different places, on the barometer, the winds, and quantity of rain.*

M. *MARALDI* having ſeen the obſervations made by Dr. *Derham* on the barometer and on the winds at *Upminſter*, in *England*, during the years 1697 and 1698, compared them with thoſe which were made at the obſervatory during the ſame years; and this is the reſult of the compariſon.

Tho' the winds which reign at *Paris*, and at *Upminſter*, are commonly different, yet there are many days in the different ſeaſons of the year, in which the winds have been the ſame in both theſe places. When the wind has been the ſame in

both, it has uſually been violent, and of ſome continuance; tho' even theſe have ſometimes varied.

There is found alſo ſome conformity in the conſtitution of the air, and it hath often happened to rain, or ſnow, or to be fair weather in both theſe places on the ſame days.

There is a great agreement between the variation of the height of the barometer obſerved at *Paris* and at *Upminſter*. We generally find it to riſe or fall at *Paris*, when it riſes or falls at *Upminſter*; tho' theſe variations are not always equal. In each month the days on which the quickſilver has been higheſt or loweſt, have been the ſame both at *Paris* and *Upminſter*; but commonly when it has been loweſt, it has been 3 or 4 lines lower at *Paris* than at *Upminſter*, the *Engliſh* meaſure being reduced to that of *Paris*.

It appears by the obſervations:

1. That the quickſilver riſes ſometimes, when the wind is N. N. E. or N. W. and that it falls with a S. S. E. and S. W. wind. It has however riſen and fallen at the ſame time in both theſe places, tho' the winds have often been different, and ſometimes even oppoſite.

2. In theſe two laſt years, when the quickſilver was at the loweſt in both places, there fell ſome ſnow; it ſometimes alſo ſnowed, tho' the quickſilver did not fall more than uſual.

3. When the quickſilver roſe, it was often fair weather, and it fell when it was rainy weather; it was often fair weather alſo when the quickſilver was low, and cloudy when it was high.

4. When the quickſilver fell at the ſame time in both places, and it rained in one and was fair in the other, the quickſilver often fell in proportion where it rained. In like manner when

it roſe in both places, it roſe higher in proportion where it was fair.

Laſtly, it appears, that the quickſilver roſe in one when it fell in the other, and that it fell in like manner, whether the wind and weather were the ſame in both places, or different.

M. *de Vauban* having ſent the academy a memoir on the quantity of rain that fell in the citadel of *Lille* for 10 years, from 1685 to 1694, M. *de la Hire* has compared the 6 laſt years of the obſervation at *Lille* with the ſame years, which he has obſerved very exactly at *Paris*.

Years.	At *Lille*.		At *Paris*.	
	Inches.	Lines.	Inches.	Lines.
1689	18	9	18	11 $\frac{1}{2}$
1690	24	8 $\frac{1}{2}$	23	3 $\frac{3}{4}$
1691	15	2	14	5 $\frac{1}{4}$
1692	25	4 $\frac{1}{2}$	22	7 $\frac{1}{2}$
1693	30	3 $\frac{1}{2}$	22	8
1694	19	3	19	9
6 Years	133	6 $\frac{1}{2}$	121	9

By the compariſon of theſe 6 years we ſee in general, that it rains rather more at *Lille* than at *Paris*; and that the mean year at *Lille* will be 22 inches, 3 lines, and at *Paris* 20 inches, 3 lines $\frac{1}{2}$.

But M. *de la Hire* has found in 1695, 19 inches, 7 lines $\frac{3}{4}$; in 1696, 19 inches, 5 lines $\frac{1}{2}$; in 1697, 20 inches, 3 lines; in 1698, 21 inches, 9 lines; and taking a mean year for theſe 10, we find 20 inches, 3 lines $\frac{1}{2}$ for each of the 6 firſt; whereas at *Lille* the 6 laſt give for a mean

mean 22 inches, 3 lines, and the 10 together give 23 inches, 3 lines.

II. *Obſervations on the ſingularities of the natural hiſtory of* France.

The academy having a deſign to examine the wonders of *France*, has begun with *Dauphiny*, and with a very famous burning fountain in that province, four hours diſtant from *Grenoble*.

Saint *Auguſtin* * has mention'd it, and ſeems to treat it as a ſupernatural miracle. But as it is good to be well aſſured of our facts, and not to ſeek for the reaſon of what does not exiſt; M. *de la Hire* wrote to M. *Dieulamant*, the king's engineer in the department of *Grenoble*, from whom he received an information, as well circumſtanced as could be wiſhed. M. *Dieulamant* went upon the ſpot, and examined it with a philoſophical eye.

It is no fountain; but a ſmall ſpot of ground, 6 feet long, and 3 or four broad, where may be ſeen a light, wandering flame, like that of brandy, fixt to a dead rock, of a ſort of rotten ſlate, which crumbles in the air. This ſpot is upon a pretty ſteep declivity; about 12 feet below it, and as much on one ſide, there falls from the neighbouring mountains a little rivulet or torrent, which perhaps may formerly have flowed more high, and near the burning earth, which might have given room to think that the water burned.

The flame is not obſerved to come out of any hole or cleft in the rock, by which it might be ſuſpected to have a communication with ſome lower cavern that might be inflamed. There is no matter found which may ſerve as nouriſhment to the flame, and it is only perceived to have a ſtrong

* *De civitate Dei*, lib. xi. cap. 7.

ſtrong ſmell of ſulphur; and it leaves no aſhes. There is a very hot, white ſort of ſalt petre about the place where the fire appears.

M. *Dieulamant* has been aſſured, that this fire burns moſt in winter, and in moiſt weather, that it diminiſhes gradually in great heats, and often goes out near the end of ſummer, after which it rekindles of itſelf. It is very eaſy alſo to make it kindle by another fire, which it does readily with a noiſe.

M. *Dieulamant* obſerved in the laſt place, that about the fire the earth cleaves, gives way, and ſinks down. He does not aſcribe this however to the fire, but to the waters which flow among the dead rocks, and hollow the earth, or carry it away. This effect is ſo conſiderable in *Dauphiny*, and eſpecially in the country called *le Chanſeaux*, that ſometimes two villages ſituated upon two different mountains, which could not be ſeen from each other, becauſe other higher mountains were between them, have begun at once to be ſeen, from the ſinking of the interpoſed mountains.

III. *Of inſects.*

Inſects appear contemptible to the vulgar, who neither know how to place their admiration nor their contempt. They are moſt commonly treated as imperfect animals, but philoſophy judges them ſo much more worthy of her attention, as they ſeem to have been formed by nature on a quite particular idea.

None but they, for example, change their ſpecies, and after having crept on the ground riſe up into the air, and take a new and more noble life. What M. *Homberg* * has obſerved of the odd

* See the memoirs for this year, Art. vii.

odd copulation of adder-bolts will ſhew how fruitful and unexhauſtible nature is in mechanical inventions, to obtain her ends.

To be of both ſexes at once, and to perform the functions of both at the ſame time, is another circumſtance peculiar to inſects. M. *Poupart* has enumerated all the ſpecies in which he is ſure of this particularity. They are the earth worms, the round worms in the human inteſtines, thoſe in the inteſtines of horſes, the ſlugs or ſnails without ſhells both of the land and the freſh waters, all the ſpecies of ſnails with ſhells, all the ſpecies of leeches. And as all theſe inſects are reptile and without bones, M. *Poupart* concludes, that probably the reſt, which have theſe two characters, are alſo hermaphrodites; for nature, which varies ſo much from one kind of animals to another, obſerves uniformity enough between the ſpecies of the ſame kind, as to their principal characters.

Not but that there are reptiles without bones, which are not hermaphrodites, as the worms which change to flies, ſilk-worms, and other animals. But far from being hermaphrodites they are of no ſex at all; and, to ſpeak properly, are not animals, being nothing but caſes or masks that wrap up and hide real animals, which afterwards come out with wings. And tho' theſe worms may ſeem to be ſenſible, yet perhaps their ſenſibility belongs only to the concealed animal, and not to that which appears. But, however, the worm which is to change to a fly or to a butterfly, is neither male nor female, and does not engender ſo long as it is a worm; but ſtays till it is metamorphoſed.

To give an example of the obſervations that may be made on the hermaphrodite animals, ſee how M. *Poupart* has made his on the copulation

of

of earth-worms. Theſe little ſerpents ſlide by pairs into a hole that ſuits them, there they adjuſt themſelves in ſuch a manner, that the head of one is turned towards the tail of another; they apply themſelves to each other in a right line, and a little button of one in form of a little cone is inſerted into a little aperture of the other reciprocally. The mutual inſertion of theſe little buttons is eaſily ſeen, by taking the two worms gently, drawing them gradually in the ſpace between the buttons, and ſurveying them in a full light. They are found in copulation in the ſpring; and you muſt look for them in fat and moiſt paſtures, if you deſire the biggeſt.

As theſe animals are male at one extremity of the body, and female at the other, and as they can eaſily bend, M. *Homberg* does not think it impoſſible, but that an earth-worm may couple with itſelf, and be both father and mother of the ſame animal. This would be a ſtrange ſort of generation; but what is ſtrange may perhaps be ſo only through our ignorance; and do we know the bounds of the diverſity with which nature has thought fit to adorn her works?

IV. *On the feathers of birds.*

M. *Poupart* has obſerved, that feathers are nouriſhed by blood and by lymph. We may be ſure of this by diſſecting with a little ſkill a great feather of a young bird before it has loſt its down; but it is ſtill more eaſy to preſs this feather lengthwiſe, and ſee the lymph and the blood come out of it. It muſt be a young bird, as it muſt always be a young animal, when one would obſerve how the nouriſhment of the bones is formed. Feathers and bones are parts whoſe veſſels diſappear, and

whoſe mechaniſm grows leſs viſible as they become more perfect. At the end of the quill there is a little hole, by which the blood-veſſels enter, juſt as they enter into a tooth by a little hole that is at the extremity of the root. The pith, which is taken from the inſide of a quill when we cut it to make a pen, in young birds is a great fleſhy canal, juſt like a vein filled with lymph, about which the blood veſſels, which entred at the end of the quill, ſpread themſelves and divide into a thouſand little branches.

But if we would know what this great fleſhy canal is, we muſt not examine it in young birds, but in ſuch as are full grown; the point of view in which it is to be placed being no longer the ſame.

An explanation of the figures.

Plate I. *Fig.* 1. The great feather of the wing of a large young bird, which has not loſt its down, being almoſt wholly incloſed in the long cartilaginous tube *a b*, which preſerves the vanes of the feather, which at preſent are only a ſort of pap.

Fig. 2. Repreſents the ſame feather, with the quill opened lengthwiſe, to ſhew in its cavity a large and long cavernous fleſhy body, *c d*, upon which are ſpread an infinite number of ſmall blood-veſſels, which enter by the lower hole *d*, to pour their lymph into the caverns of this body, to be carried into all the parts of the feather to nouriſh them.

Fig. 3. The pith taken out of the quill of a large young bird, to ſhew them more eaſily and diſtinctly.

Fig. 4. Cavernous bodies compoſed of funnels towards the upper part, and cups in the reſt,

which are ſeen in the great quills of young birds, when they begin to dry and become tranſparent.

Fig. 5. Three funnels boxed into one another, which are taken out of the canal *a* of *Fig.* 4, to ſhew how the canals of theſe funnels enter afterwards into their pavilions, to carry the lymph into the veins and pith of the feather *Fig.* 2. to which the canal *e* is faſtened at the place *c*.

Fig. 6. Little bags or cups of which the body repreſented in *Fig.* 4. is chiefly compoſed.

We ſee then in grown birds, that this canal is compoſed of ſeveral little tranſparent cups, placed one above another, and diſpoſed in ſuch a manner, that the bottom of the lower one is articulated, or faſtened into the opening of the upper one, and ſo upwards on to the top of the quill. But when we come near to the vanes of the feather, theſe little cups become like funnels, at leaſt in ſome ſpecies of birds, as in turkeys. The tube of the lower funnel enters into the pavilion of the upper one, and faſtens itſelf to the bottom of it, and the tube of the laſt funnel enters into the pith of the feather.

The blood-veſſels pour their lymph into theſe little cups, and thence it is filtred to the top of the quill, whence it enters into the pith of the feather, which being only a ſpongy ſubſtance eaſily imbibes it, and diſtributes it on both ſides into the vanes.

In turkeys this pith is nothing but an aſſemblage of an infinite number of pretty ſenſible canals; for the parts of the organs of an animal are themſelves organiſed, and the complication of the mechaniſm is infinite; and there is hardly any room to queſtion, but that the other birds whoſe canals of the pith of the feather are not viſible,

are found there only in ſmall, and perform the ſame functions.

M. *Poupart* has obſerved that one ſingle feather of a young vultur, with the down ſtill on, weighed more than ſix other feathers of the ſame bigneſs that were in their perfection, it was ſo loaded with nutritious juices; and thence he concludes, that as feathers are inſtruments abſolutely neceſſary for birds to ſeek their ſubſiſtance, nature makes haſte to bring them to perfection, and labours about them with more diligence than ſhe does in moſt of her other works.

One might be contented with ſeeing that the cavity of the quill had been wrought by nature, to reconcile at once ſtrength, ſuppleneſs and lightneſs; but we ſee beſides that this void ſerves for a magazine to the nouriſhment which is to be diſtributed thro' the whole feather, and that the ſame mean anſwers to ſeveral views at once.

It is another curioſity, to obſerve the care that nature takes to preſerve the ſprouting feathers of young birds. The vanes of theſe feathers are at firſt ſo tender and delicate, that they ſeem to be meer pap. They are alſo rolled up in a long cartilaginous tube, full of moiſture, that they may not be expoſed to the air, which would ſo dry them and ſhut up their pores, that they could not receive any nouriſhment. But when they are ſtrong enough to be no longer afraid of the action of the air, the caſe, in which they were rolled up, becoming no longer neceſſary, dries up and falls off of itſelf in ſcales.

V. *Of the tigers in* China.

F. Gouye has communicated an anatomical deſcription of a ſtriped tiger, made at *China* by the

the *Jeſuits*. We hardly know in *Europe* any tigers, but ſuch as have their ſkin marked with ſpots; but in *Tartary* and *China* they have a ſort which is marked with black ſtripes; and in thoſe countries it is pretended that they are two different ſpecies, tho' they do not ſeem to have any other difference. The ſtriped tiger, which the *Jeſuits* of *China* diſſected, had been killed in hunting by the emperor, together with four others: It weighed but 265 pounds, ſo that it was none of the biggeſt; one of the others weigh'd 400 pounds. That which was diſſected, had $\frac{1}{3}$ of its ſtomach full of worms, and it could not be ſaid to be corrupted: One of the company ſaid he had found the ſame thing in another tiger, which he had ſeen opened at *Macao*.

VI. *The poſition of ſome towns in* China, Turkey, *and* Armenia.

F. *Gouye* having collected the obſervations made by the *Jeſuits* in theſe countries, has calculated them all, and drawn from them the poſitions of ſeveral towns: He has choſen the leaſt doubtful operations, has had regard to the error which may be ſuſpected in the inſtruments, he has taken the mean numbers between the greateſt differences, which reſult from different obſervations made for the ſame end.

We ſhall not give here either the obſervations of the *Jeſuits*, or the calculations of F. *Gouye*, but only the reſults of them.

The longitudes were taken from the immerſions or emerſions of the ſatellites of *Jupiter*, which F. *Gouye* has reduced to the meridian of *Paris*, by M. *Caſſini*'s tables, and by the neareſt obſervations

vations which he has found to have been made at the obſervatory.

The poſitions of ſome towns in China.

Pekim.

	°	′	″
Lat. N. at the houſe of the *Jeſuits*	39	54	
Longitude - -	136	46	30
It muſt be obſerved, that we here ſuppoſe, and always ſhall hereafter ſuppoſe the longitude of *Paris* to be - -	22	3	

Ning-po or Liampo, in the province of Chekiam.

	°	′	″
Lat. N. - -	29	56	
Longitude	141	18	
The cape of *Ning-po* is about 5 leagues more to the E. than *Ning-po*, which gives almoſt 15′ of a degree in longitude.			
The *Cape-Vert* is more W. than *Paris* by - -	19	30	
Which gives the difference in longitude between *Cape Vert* and *Ning-po* - -	138	33	
However F. *Coronelli* in his great globe makes it - -	163	37	

Kiam-Cheu, a town of the firſt rank in the province of Xanſi.

	°	′	″
Lat. N. - - -	35	37	
Longitude - - -	131	39	15

Nan Kim.

Lat. N. - -	32	4	
Longitude - -	139		

Xambay,

Xamhay, a town of the third order on the E. coaſt of China, *and upon a great river named* Hoampou, 4 *leagues from the ſea,*

	°	′	″
Lat. N. - - -	31	16	

The obſervations from which this latitude was taken, are not abſolutely certain. There may be 2 minutes difference.

Longitude - - -	141	41	45

Sing-ghan-fu, capital of the province of Xenſi.

	°	′	″
Latitude N. - -	34	16	45
Longitude - - -	129	6	45

Nam-cham-fu, capital of the province of Kiamſi.

	°	′
Latitude. - -	28	40

Kam-cheu-fu of Kiamſi.

Latitude - - -	28	49	54

Nan-ghan-fu of Kiamſi.

Latitude - - -	25	27	31

Xoacheu.

Latitude - -	24	44	10

It is doubtful.

Canton.

Latitude - - -	23	7	46
Longitude - -	133	13	15

Tho' the latitude is different from all thoſe that have hitherto been aſſigned to this city, yet we may depend more upon this, becauſe it is drawn from the moſt exact and numerous obſervations.

Su-cheu-fu, in the province of Namkim.

	°	′	″
Latitude - - -	31	17	50
Longitude - - -	140	16	15

The

The obſervations, from which F. *Gouye* has concluded theſe latitudes and longitudes, which will ſerve to rectify the map of *China*, are received from the Fathers *de Fontanay*, *le Comte*, *Bouvet*, *Gerbillon*, and *Viſdelou*.

The academy received alſo in *June* a letter from F. *de Fontenay*, dated *Nov.* 8. 1697, at *Tchaotcheou*, a city of the firſt order in the province of *Canton*. He gave an account of an obſervation he had made of *mercury* in the ſun at *Tchaotcheou*, *Nov.* 3, 1697, Tho' he did not think it himſelf entirely exact, F. *Gouye* calculated it, in order to compare it with the obſervation at *Paris*. He found by this compariſon, that the total emerſion of *mercury* from the diſk of the ſun, which was ſeen at *Tchaotcheou* at $3^{h}\ 48'\ 44''$ afternoon, was ſeen at *Paris* at $8^{h}\ 10'\ 24''$ in the morning, and that conſequently the difference of the meridians of *Tchaotcheou* and *Paris*, is $7^{h}\ 38'\ 20''$, which being valued in degrees, and added to the longitude of *Paris*, gives the longitude of *Tchaotcheou*

	137° 5' 30"
F. *Fontenay* judges alſo by eſtimation, that *Tchaotcheou* is more eaſterly than *Canton* - - -	3 30
Whence the longitude of *Canton* would reſult - - -	133 35 30
It has been found by the *Satellites* to be - - -	133 13 15

Which is a ſufficient conformity for ſuch great diſtances, and in ſuch nice operations.

The

Fig. 1.

Fig. 2.

Fig. 3.

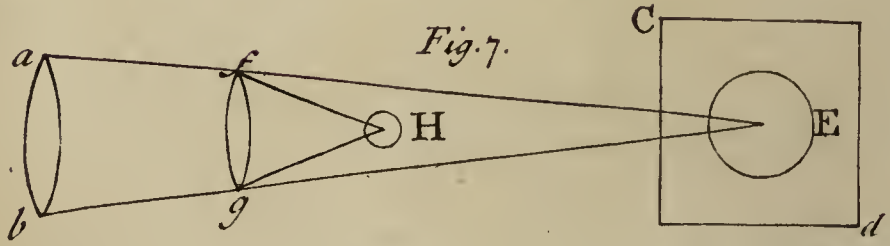
Fig. 4.

Fig. 5.

Fig. 6.

Fig. 6.

Fig. 7.



J. Mynde sc.

The positions of some towns of Turkey *and* Armenia.

Smyrna.

	°	′	″
Latitude	38	12	30
Coronelli makes it	39	15	

Trebisonde.

Latitude	41	3	54
Riccioli makes it	43	11	
Longitude	65	0	45
Coronelli makes it	71	30	

Erzeron.

Latitude	39	56	35
Longitude	68	45	45

Eruan.

Latitude	40	19	33

F. *Gouye* has drawn these conclusions from the observations of F. *de Beze*.

VI. *The effects of burning glasses of three or four feet in diameter.*

M. *Tschirnhaus* has cut some burning glasses, the effects of which are above all that has yet been seen. What he has communicated to us is as follows.

1. You must place the glass *a b*, * so that it may receive the rays of the sun full, which may be known by the *focus* or image of the sun E, appearing perfectly round on the plank *c d*, which must be parellel to the glass *a b*; and if this figure should be oval, you must bend and turn the glass till it arrives at this perfect roundness; and then wood will catch fire in a moment, lead will

* Plate I. Fig. 7.

melt, ſlates will vitrify, *&c.* and this at the diſtance of 12 feet, the focus being 1 inch ½ in diameter.

To render this *focus* more vivid, you muſt contract it by means of a ſecond *lens*, which muſt be placed at *f g*, parallel to the firſt glaſs *a b*, and then the *focus*, which was at E, will be at H, and whereas it had a diameter of 1 inch ½, it will have one of 8 lines; but its force will be much augmented, ſo that the ſubſtances, which were not fuſible at E, will melt at H.

3. This glaſs muſt be mounted on a foot like that which bears the great burning mirrour of the obſervatory, that it may be turned every way, and placed in a convenient ſituation to receive the rays of the ſun always full upon the glaſs. Theſe rays are always ſtrongeſt from 9 in the morning to 3 in the afternoon, both in ſummer and winter. It muſt be obſerved alſo to expoſe leſs matter in winter than in ſummer, and that the effects are a little ſlower in winter. Theſe effects are:

1. Every ſort of wood, let it be ever ſo hard, or ever ſo green, nay, tho' it be ſoaked in water, will catch fire in a moment.

2. Water in a ſmall veſſel will boil in a moment.

3. Bits of metal being of a proportionate bigneſs will melt, not in a moment, but immediately after the whole bit of metal has attained to a certain degree of heat. For example, a bit of lead, if it is too big, will not melt at all, but being of a proportionate bigneſs, it muſt be held a little while in the *focus*; and when it begins to melt in one place, all the reſt will continue to melt. The iron muſt be in very thin flakes, and

then

then it will grow red in a moment, and afterwards melt alſo.

4. Tiles, ſlates, pumice-ſtones, earthen ware, *&c.* of any bigneſs whatſoever, grow red in a moment and vitrify.

5. Sulphur, pitch, and all ſorts of reſins melt under water.

6 When you expoſe to it in ſummer any tender wood, ſuch as fir, under water, it does not ſeem to change it on the outſide; but when it is cleft in two, it is found burnt within to a coal.

7. If you make a hollow in a coal of hard wood, and put the ſubſtances into it that you would expoſe to the ſun, the effect will be infinitely more violent.

8. Any metal whatſoever put into the hollow of a charcoal melts in a moment, and fire ſparkles in it as in a forge; and if you keep the metals in this manner in fuſion for ſome time, they fly quite away, which happens particularly and very readily to lead and tin.

9. The aſhes of wood, herbs, paper, linnen, *&c.* turn to a tranſparent glaſs in a moment.

10. If any ſubſtances will not melt when they are in pieces, you muſt expoſe them in powder; and if they do not melt in powder, you muſt add ſome ſalt, and the whole will melt.

11. The ſubſtances, which are moſt altered by this fire, are ſuch as are black, and remain black in melting; the more difficult are ſuch as are white and become black in melting; ſtill more difficult are thoſe which are black and become white in melting; the moſt difficult of all are the white ſubſtances, which remain white in melting, ſuch as flints, *Engliſh* chalk, lime, *&c.*

12. All metals vitrify on a *China* plate, provided it is thick enough not to melt itſelf, and the fire is given it by degrees that it may not crackle. Gold receives a fine purple colour in its vitrification.

13. If you put in a great receiver *a b c* ſome ſubſtances in *c* which melt eaſily, ſuch as herbs, ſulphur, antimony, zink, biſmuth, *&c.* by applying a ſingle burning glaſs, you may obſerve very curious effects in the receiver; but you muſt take care that the part of the receiver *a b*, which gives paſſage to the rays of the ſun, may not be near enough to the *focus c*, for the heat to break the receiver.

14. Salt petre in a ſuitable doſe volatiliſes entirely, and goes off in ſmoke; ſo that by this means one might make ſpirit of nitre readily in a great receiver.

15. To melt the greateſt quantity of matter at once, you muſt put but a little at a time, and when it is melted, add a little more, and ſo on. In this manner, you may keep 4 ounces of ſilver in fuſion at once.

16. A ſolid ſubſtance which is eaſily put in fuſion, may ſerve as a flux to another, which is melted with difficulty, if you expoſe them to the *focus* together, even tho' there might be but a little of that which is eaſily fuſible.

17. It is obſervable alſo, that two ſubſtances, each of which is difficult to melt ſeparately, when they are expoſed together in a certain doſe, melt very eaſily, as flints and *Engliſh* chalk.

18. A little copper melted in this manner, and thrown quickly into cold water, produces ſo violent a ſtroak in this water, that the ſtrongeſt earthen veſſels break, and the copper flies away divided into ſuch ſmall parts, that there cannot

the leaſt grain be found, which does not happen to any other metal.

19. As metals evaporate in the fuſion, ſome ſooner than others, ſome of them may thereby purify others; for example, ſilver may be purified by lead, as well as by the ordinary cuppel.

20. You may make by it all ſorts of coloured glaſs.

21. All bodies, except metals, loſe their colours in this fire, and even precious ſtones are quickly deprived of theirs; ſo that an oriental ruby loſes all its colour in a moment.

22. Some bodies vitrify immediately, and become as tranſparent as cryſtal, and when cool they become as white as milk, and loſe all their tranſparence.

23. Some bodies on the contrary, which are opake in the fuſion, become finely tranſparent when they are cold.

24. Some ſubſtances are very tranſparent in the fuſion, and remain ſo when cold; but ſome days after they become opake.

25. Some ſubſtances, which the fire changes into a glaſs, that is tranſparent at firſt, and afterwards becomes opake, being melted with other ſubſtances that are always opake, will produce a fine glaſs, which will always remain tranſparent.

26. Thoſe bodies, which change into a tranſparent glaſs, become much more beautiful, if they are left a little longer in the *focus*.

27. Some ſubſtances turn to ſo hard a glaſs, that being cut in facets, it cuts common glaſs.

28. When you melt lead and tin together on a thick plate of copper, there comes out a great deal of ſmoke; but if there is only one of them, and they do not go off entirely in ſmoke, there remains always a vitrified *ſcorium*.

29. You

29. You may concentrate the rays of the moon by theſe glaſſes, but they will give no ſenſible heat, tho' they make a great brightneſs.

30. You may make alſo with theſe glaſſes curious repreſentations in opticks, and better than with the concave mirrours; and you may make alſo glaſſes for teleſcopes and microſcopes, much better than any that have hitherto been ſeen.

VII. *On a machine made to prove the proportion of the fall of bodies.*

The ingenious hypotheſis of *Galileo*, on the proportion according to which the quickneſs of falling bodies increaſes, is from henceforth eſtabliſhed in phyſicks. By dividing into equal parts the time of the fall of a body, we know that in the ſecond moment it goes thro' 3 times more ſpace than in the firſt, 5 times more in the third, and ſo on, according to the odd numbers. But this hypotheſis, ſo convenient for calculation, and ſo probable in reaſoning, is however not good at the bottom, any otherwiſe than as it agrees with experience, and this is not very eaſy to prove in a great degree of exactneſs.

For this purpoſe, F. *Sebaſtien* has conceived and very exactly executed a machine, which ſeemed to him more fit to prove the ſyſtem of *Galileo*, than the other experiments which have been made with the ſame deſign.

This machine is compoſed of 2 or 4 equal parabolas, which interſect at their top by making equal angles, and have a common *axis* perpendicular to the horizon. This forms a paraboloid, about which there turns a ſpiral, compoſed of two parallel braſs wires, which make a very narrow inclined plane, and ſo diſpoſed,

poſed, that the firſt turn of the ſpiral being 1 inch in diameter, the ſecond is 3, the third 5, the fourth 7, *&c.* Theſe turns of the ſpiral, which are to each other as their diameters, are the unequal ſpaces which the falling bodies muſt go thro' in equal times. We ſee therefore, that if we let a little ivory ball of 6 inches in diameter fall from the top of the paraboloid, and paſs thro' the whole inclined ſpiral plane, it will paſs thro' all the turns in the ſame time; and this will appear ſtill better, if 2 equal balls turn about a paraboloid at the ſame time, and at ſome diſtance from each other; for when one has ſeen them paſs in the ſame inſtant over the ſame arch of one of the parabolas, one ſees that they continue to go always together, and to be found at the ſame inſtant over every other arch whatſoever, tho' being at different heights they go through very unequal turns of the ſpiral.

AN

ABRIDGMENT

OF THE

Philoſophical Memoirs

OF THE

HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris* for the Year 1699 *.

I. *Reflections on light and colours, and on the generation of fire, by F.* Mallebranche.

IN order to explain my notion of the natural cauſes of light and colours, let us imagine a great ball preſſed on the outſide by an infinite force, and filled with a fluid matter, the motion of which is ſo rapid, that not only the whole turns with a great deal of quickneſs, about a common centre, but alſo, that each part, to fulfil its whole motion, that is, to move as much as it has force to do ſo, is obliged either to turn upon the centre of an infinite number of little *vortices*, or elſe to flow among them, and all this with an extraordinary rapidity. In a word, let us imagine the matter contained in this ball to be nearly ſuch, as M. *Deſcartes* has aſſigned to our *vortex*; excepting that the globules of his ſecond element, which he ſuppoſes to be hard, are themſelves only little *vortices*, or at leaſt that they have no hardneſs, but by the compreſſion of the matter

* April 4. 1699.

matter which ſurrounds them. For if theſe globules were hard of themſelves, which I think I have ſufficiently proved * to be falſe, they could not, as we ſhall ſee hereafter, tranſmit the light and different colours thro' the ſame point, where the rays croſs. But if this ſuppoſition meets with any difficulty, it is ſufficient at preſent to imagine a ball full of water, or rather of a matter infinitely fluid, and extremely compreſſed on the outſide. The circle A B C † is the ſection of this ball thro' the centre.

This being ſuppoſed, if we make a little hole in this ball, as at A, I ſay, that all the parts of water, as thoſe, for inſtance, at R, S, T, V, will tend towards the point A, in right lines RA, SA, TA, VA. For all theſe parts, which were equally preſſed, ceaſing to be ſo on the ſide which anſwers to the hole A, they muſt tend thither; ſince every body that is preſſed muſt tend to move itſelf by the ſide where it finds the leaſt reſiſtance.

But if we put a piſton to the aperture A, and drive it quickly in, the ſame parts R, S, T, V, *&c.* will all endeavour to remove from the hole by the ſame right lines A R, A S, *&c.* becauſe when the piſton advances, they are more preſſed by the ſide which anſwers to them directly, than by any other.

In fine, if we conceive that the piſton advances and retires very quickly, then all the parts of the fluid matter which fills the ball exactly, the elaſticity of which I ſuppoſe to be very great, or that it ſtretches or extends itſelf with great difficulty, will receive an infinite number of ſhocks, which I call *vibrations of preſſure.*

* *Ch. Dernier de la Recherche de la Verite.*
† Plate II. Fig. 1.

As every thing is full, our eyes are actually preſſed, tho' ſhut or in the dark. But this compreſſion of the optic nerve does not excite any ſenſation of colours, becauſe this nerve is always equally compreſſed: for the ſame reaſon that we do not feel the weight of the air which ſurrounds us, tho' as heavy as 28 inches of quickſilver. But, if we imagine an eye at T, or elſewhere, turned towards a flambeau at A; the parts of the flame being in a continual motion, will preſs without ceaſing more ſtrongly than in the dark, and by very quick ſhakes or vibrations, the ſubtile matter on all ſides, and conſequently becauſe of the *plenum*, quite to the end of the eye; and the optic nerve, being more preſſed than uſual, and ſhaken by theſe vibrations, will excite in the mind a ſenſation of light, and of a lively and glaring whiteneſs.

If we ſuppoſe at S, a black body M; the ſubtile matter not being reflected towards the eye turned that way, and not ſhaking the optic nerve, we ſhall ſee black, as when we look into the mouth of a cave, or the hole of the ſight of an eye.

If the body M is ſuch, that the ſubtile matter, which the flambeau ſhakes, is reflected from this body towards the eye, without the quickneſs of the vibrations diminiſhing, this body will appear white, and ſo much the more white, as there are more rays reflected. It will alſo appear luminous like the flame, if the body M being poliſhed, the rays are all reflected, or a great part of them, in the ſame order, becauſe the brightneſs comes from the force of the vibrations, and the colour from their quickneſs.

But if the body M is ſuch, that the ſubtile matter being reflected has its vibrations leſs quick

in

in certain degrees, which I do not think we can determine exactly; we ſhall have ſome one of the colours which are called *primitive*, yellow, red, or blue, if all the parts of the body M equally diminiſh the vibrations cauſed by the flame in the ſubtile matter. And we ſhall ſee all the other colours which are formed by the mixture of the primitive, according as the parts of the body M ſhall unequally diminiſh the quickneſs of the vibrations of the light. This is what I meant, when I advanced in ſome of my books, that light and colours conſiſted only in *different ſhakes* * *or vibrations* of the etherial matter, or in † *more or leſs quick vibrations of preſſure*, which the ſubtile matter produced on the *retina*.

This plain diſcovery of my opinion will perhaps make it appear ſomething probable; at leaſt to thoſe who are acquainted with the philoſophy of M. *Deſcartes*, and are not contented with the explication which that learned man has given of colours. But that a more ſolid judgment may be formed of my opinion, it will be neceſſary to give ſome proof of it; and therefore we ſhall obſerve:

1. That ſound is heard only by the *medium* of the vibrations of the air, which ſhakes the auditory nerve: for when we have drawn the air as much as we can out of the pneumatic engine, the ſound does not paſs thro' it, but is louder as the air is condenſed, and leſs in proportion as it is rarefied.

2. That the difference of tones does not come from the force of the vibrations of the air, but from their greater or leſs quickneſs, as all the world knows.

* *Recherche de la Verite*, tom. 2. pag. 364.

† *Entretiens ſur la Metaph* 12. *Entr. n.* 1.

3. That tho' the impreſſions, which objects make on the organs of our ſenſes, differ ſometimes only as to more and leſs, yet the ſentiments, which the mind receives from them, differ eſſentially. There are no ſenſations more oppoſite than pleaſure and pain; yet he that ſcratches with pleaſure, will feel pain if he ſcratches a little harder: becauſe the greater or leſs motion in our fibres differs eſſentially with reſpect to the well being of the body, and our ſenſes inform us only of this relation. It is very probable, that ſweet and bitter, which cauſe ſuch oppoſite ſenſations, often differ only as to more and leſs; for there are ſome who think bitter what others think ſweet. There are ſome fruits which to-day are ſweet, and to-morrow will be bitter. A little difference therefore in the bodies renders them capable of cauſing very oppoſite ſenſations. In a word, the laws of the union of the ſoul and body are arbitrary, and there is nothing in the objects like the ſenſations that we have of them.

It is certain, that colours depend naturally on the ſhaking of the organ of the ſight. Now this ſhaking can be only ſtrong and weak, or quick and ſlow. But experience ſhows, that the more or leſs ſtrength or weakneſs of the ſhaking of the optic nerve does not alter the ſpecies of the colour; ſince the more or leſs light, on which depends the more or leſs of this force, does not uſually ſhew the colours of a different and quite oppoſite ſpecies. It is therefore neceſſary to conclude, that it is the more or leſs quickneſs in the vibrations of the optic nerve, or of the ſhocks of the ſpirits contained in them, that changes the ſpecies of colours, and conſequently that the cauſe of theſe ſenſations comes primitively from the

the *more or leſs quick vibrations of the ſubtile matter*, which compreſſes the *retina*.

Thus it is with light and different colours, as it is with ſound and different tones. The *greatneſs* of the *ſound* comes from the more or leſs *force* of the vibrations of the groſs air, and the diverſity of the tones from the more or leſs *quickneſs* of the ſame vibrations, as all agree. The *force* or brightneſs of the colours comes therefore alſo from the more or leſs *force* of the vibrations, not of the air, but of the ſubtile matter, and the *different ſpecies of colours* from the more or leſs *quickneſs* of the ſame vibrations.

When one has looked at the ſun, and the optic nerve has been very much ſhaken by the brightneſs of its light, becauſe the fibres of this nerve are ſituated in the *focus* of the tranſparent humours of the eye; then if one ſhuts the eyes, or enters into a dark place, the ſhaking of the optic nerve will alter only as to more and leſs. However, we ſhall ſee different colours, white at firſt, yellow, red, blue, and ſome of thoſe which are formed by the mixture of the primitives, and in the laſt place black. Whence we may conclude, that the vibrations of the *retina*, which are very quick at firſt, become ſlower by degrees. For once more, it is not the greatneſs or *force* of theſe vibrations, but their *quickneſs* that changes the *ſpecies* of the colours; ſince red, for example, appears red to a weak as well as to a ſtrong light. We might therefore judge perhaps by the ſeries of theſe colours, if it was very conſtant, that the vibrations of the yellow are more quick than thoſe of the red, and of the red than of the blue, and ſo of the other colours that ſucceed. But it ſeems to me impoſſible to diſcover preciſely by this means, or even by any other, the exact relations

of

of the quickneſs of theſe vibrations, as they have been diſcovered in the conſonances of muſick. We can therefore only conjecture, and have recourſe to the probable.

As the air is compreſſed only by the weight of the atmoſphere, there muſt be ſome little time allowed for every particle of air to remove the next to it. Therefore ſound tranſmits itſelf pretty ſlowly. It travels no more than about 180 toiſes in a ſecond. But it is otherwiſe with light, becauſe all the parts of the ethereal light touch one another, and are very fluid, and eſpecially, becauſe they are compreſſed by the weight, if I may ſo ſpeak of all the *vortices*. So that the vibrations of preſſure, or the action of the luminous body, muſt communicate from a great diſtance in a very little time. And if the compreſſion of the parts, which compoſe our *vortex*, was infinite, then the vibrations of preſſure muſt be made in an inſtant.

M. *Huygens*, in his treatiſe of light *, concludes from the eclipſes of the *Satellites* of *Jupiter*, that light tranſmits itſelf about 600,000 times faſter than ſound. Thus the weight or preſſure of the whole celeſtial matter is without compariſon greater than that which is produced on the earth by the weight of the atmoſphere. I think I have ſufficiently proved elſewhere †, that the hardneſs of bodies can come only from the preſſure of the ſubtile matter. And if this is ſo, it muſt be extremely great, ſeeing ſome bodies are ſo hard, as to require a very great force to ſeparate the leaſt parts of them. It appears to me, that the proportion of the weight of the *æther* to that of the atmoſphere, is yet greater than that of 600,000

* *Page* 9.

† *Recherches de la Verite, Ch. dernier.*

to 1, and that we may even look upon it as infinite.

Let us therefore now ſuppoſe, that all the parts of the *æther*, or ſubtile and inviſible matter of our *vortex* are compreſſed with an infinite force by thoſe which encompaſs it, and that each of its parts is very fluid, and has no hardneſs but by the motion of thoſe which encompaſs or compreſs it on all ſides. And let us ſee how it is poſſible in the ſyſtem that I propoſe, that the impreſſions of an infinite number of rays, or of different colours, communicate without confounding each other. Let us ſee how 10,000 rays, which croſs each other in a phyſical, or ſenſible point, tranſmit all their different vibrations thro' the ſame point, ſince I have juſt proved, that the difference of colours cannot come but from the more or leſs quickneſs of the ſame vibrations. Probably the ſyſtem of the world, which may clear up this grand difficulty, will be conformable to the truth.

Let * A P E N be the ſection of a chamber painted with an infinite variety of colours, and let them be the moſt diſtinct that can poſſibly be; that is, let there be at A white next to the black *n*; blue *b* next the red *r*; yellow *i* next to the violet *v*. If from all theſe points A, *n*, *b*, *r*, *i*, *v*, there be drawn right lines interſecting each other as in Q, and the eye be placed beyond as at E, *c*, *d*, *f*, *g*, *h*, we ſhall ſee all theſe different colours by the intermiſſion of the point of interſection Q: and as this figure repreſents only one row of colours, whereas we ought to imagine as many as there are parts that the eye can diſtinguiſh in a ſphere, the point of interſection Q muſt receive

* Fig. 2.

and

and tranſmit a very great number of different impreſſions, without their deſtroying each other.

If the phyſical point, or the globule Q was a hard body, as M. *Deſcartes* ſuppoſes, it would be impoſſible for the eye at E to ſee the white at A, and another eye at *e* to ſee the black at *n*. For when a body is perfectly hard, if ſome part of this body advances a little, or tends directly towards the optic nerve, as from A towards E, it is neceſſary that all the other parts tend thither alſo. Then we ſhall not be able to ſee the black and the white at the ſame time by the rays that interſect in Q.

M. *Deſcartes* pretends again, that the red is formed by the turning of little globules, which communicate together in the whole ray from the object to the eye. This opinion is inſupportable for many reaſons. But it is enough to overthrow it, if we do but conſider, that if the globule Q turns upon the *axis* P N from *r* where there is red, to *f* where the eye is, it will not be able to turn at the ſame time upon the *axis r f*, from N where I again ſuppoſe red, to P where I ſuppoſe another eye.

Moreover, when I ſay that the rays interſect in the globule, or *vortex* Q, I do not pretend that theſe little *vortices* are exactly ſpherical, nor that the viſible rays have the ſame thickneſs with a globule of the ſecond element or a *vortex*. I do not determine what muſt be the bigneſs of theſe rays, for them to be able to ſhake the optic nerve ſufficiently to ſhew the colours. But what I have ſaid of one ſingle globule, muſt be underſtood of a thouſand, or a million, if one ray, in order to be ſenſible, muſt be extended as far as a thouſand or a million of globules.

It is not therefore poſſible, that the globule Q, or its like, can tranſmit the action of the light proper to ſhew all ſorts of colours, ſuppoſing theſe globules to be hard. But if we conceive them infinitely fluid or ſoft, ſo that the ſimple idea of the matter repreſents all the bodies, ſince reſt has no force, and it is indifferent to each part of a body to be or not to be near its neighbour, and it muſt part from it eaſily, if ſome force, that is, ſome motion does not reſtrain it; for we do not conceive in the bodies any other force than their motion: if, I ſay, we imagine theſe globules very ſoft, or rather, which I think is the truth, like little *vortices*, compoſed of a matter almoſt infinitely fluid, or extremely agitated, they will be ſuſceptible of an infinite number of different impreſſions, which they may communicate to others on which they reſt, and with which they are almoſt infinitely compreſſed. This I muſt endeavour to explain and prove.

In order to this, it is neceſſary perfectly to comprehend, that the re-action, which like the action communicates itſelf at firſt in a right line, is here neceſſarily equal to the action: by this reaſon eſſential to the effect in queſtion, that our *vortex* is almoſt infinitely compreſſed, and that conſequently it cannot have any *vacuum* in it. If, for example, I was to puſh my cane againſt an immoveable wall, my hand and cane would be repelled with the ſame force which I puſhed with. The reaction will be equal to the action. Now, tho' the rays are not hard like ſticks, the ſame thing happens with regard to the reaction, becauſe of the preſſure and plenitude of our *vortex*.

For if we ſuppoſe a barrel full of water, or the ball of the firſt figure full of air, and that having adapted a pipe to it, we puſh a piſton into the

pipe, this piſton will be as much repelled as it is puſhed forward. And if we make beſides in this piſton a little hole, for the water to get out of the barrel, and puſh this piſton, all the water, that ſhall be compreſſed by it, will tend at the ſame time, becauſe of its fluidity, to remove itſelf from each point of this piſton by the action, and by the reaction it will approach the hole in the middle. For if the piſton was puſhed with ſufficient force and quickneſs, the barrel would burſt in the weakeſt place, whereſoever it was, a certain mark, that by the action of the piſton, the water would preſs the barrel every where, and if the piſton is puſhed ever ſo little, the water will immediately ſpout out of the little hole in conſequence of the reaction. And all this, becauſe the reaction is equal to the action in the *plenum*, and the water or ſubtile matter is ſoft enough or fluid enough for each part to form itſelf in ſuch a manner as to anſwer all ſorts of impreſſions.

It muſt be obſerved, that the more ſtrongly we puſh the piſton that is let into the barrel, by ſo much more is the water, tho' driven toward the concave ſurface of the barrel, ſtrongly repelled toward the piſton, and ſpouts at its aperture with the more force. Whence it is eaſy to judge, that a black point on a white paper muſt be more viſible than on a blue paper: becauſe the white returning the light more ſtrongly than any other colour, not only ſhakes the optic nerve very much, but is the cauſe that the ſubtile matter tends by the reaction toward the black point with more force; but if the ethereal matter was not infinitely ſoft or fluid, it is clear, that the globules which tranſmit the impreſſion of the white, being hard, muſt hinder that of the black; becauſe theſe globules ſuſtaining each other, could not

tend

tend toward the black point: and if this ethereal matter was not compreſſed, there would be no reaction.

What I have juſt ſaid of the white and black, may be applied to the other colours. But it would be very difficult to do it particularly, and to anſwer the difficulties which many people might form on this ſubject; for it is eaſy to make objections on obſcure ſubjects. I think however, that I have clearly proved, that the *different colours* conſiſt only in the different *quickneſs* of the vibrations of preſſure of the ſubtile matter; as the *different tones* of muſick come only from the *different quickneſs* of the vibrations of the groſs air, as we learn from experience, which vibrations alſo croſs without deſtroying each other.

It ought not to be imagined, that what I have ſaid of the globules of the ſecond element, which far from thinking hard, I rather look upon as little *vortices* of a fluid matter, can overthrow the philoſophy of M. *Deſcartes*. On the contrary, if my opinion is true, it perfects what is general in his ſyſtem. For if my opinion may ſerve to explain light and colours, it ſeems to me alſo very proper to reſolve, conformably to the principles of this philoſopher, other general queſtions in natural philoſophy; as for example, to explain the generation and ſurpriſing effects of fire, which I ſhall now endeavour to do.

On the generation of fire.

As bodies cannot naturally acquire any motion, unleſs it is communicated to them by ſome others, it is plain, that fire cannot be kindled without the communication of the motion of the ſubtile matter to the groſſer bodies. M. *Deſcartes*, we know, ſuppoſes, that it is only the firſt element that com-

municates its motion to the third, of which the groſſer bodies are compoſed, and which by agitating them ſets them on fire. According to him, when we let off a gun, we forcibly detach a little part of the flint. (I ſhould rather it was a part torn from the ſteel that kindled; for when we examine with a microſcope the ſparks of fire gathered together, we ſee that it is the ſteel that has been diſſolved and reduced to globules, or little ſerpents; and I have not obſerved any alteration to be made in the ſparks detached from the flint.) This little part being detached from the iron whirling round with force, drives the globules of the ſecond element, and makes the firſt flow back upon it, which encompaſſing it on all ſides, communicates to it a part of its rapid motion which makes it appear in fire. This is pretty nearly the opinion of M. *Deſcartes* on the generation of fire. But if the globules are hard, and all of them touch, as he ſuppoſes for the explication of colours; it is difficult to comprehend how the firſt element could flow back towards the part detached from the iron; and that with a ſufficient abundance to encompaſs it and ſet it on fire; and not only it, but all the powder of a cannon or of a mine, the effects of which are violent. For the firſt element that can flow back can be at moſt but a very ſmall portion of the ſubtile matter, which fills the little triangular and hollow ſpaces, which the globules leave between them.

I ſhall therefore thus explain the generation of fire and its violent effects, on the ſuppoſition that the globules of the ſecond element are in effect only little *vortices*, of a fluid matter very much agitated.

But it muſt firſt be obſerved, that tho' the air is not neceſſary to excite any little ſpark of fire, yet without

without air the fire ſoon goes out, and cannot ſo much as communicate itſelf to the gun-powder, which is ſo ready to catch fire. When we diſcharge in *vacuo* a piſtol well primed, we learn by experience, that for want of air the prime does not take, and that it is very difficult to ſee ſo much as one ſpark from it. In ſhort, every one knows that fire goes out for want of air, and kindles by blowing. This being ſuppoſed, I explain the generation of fire, and its quick effect in mines after the following manner.

If one lets off a firelock *in vacuo*, the force of the ſtroke beats off a ſmall part of the iron or ſteel. This little part whirling round, and ſtriking quickly on ſome little *vortices* of the ſecond element, breaks them, and conſequently determines their parts to encompaſs, and afterwards to agitate it and ſet it on fire. But the matter of theſe *vortices*, which we cannot imagine too much agitated, after having had in an inſtant a great number of irregular motions, partly replaces itſelf quickly in new *vortices*, and partly eſcapes into the intervals of the ſurrounding *vortices*, which intervals become greater, when theſe *vortices* approach the detached part of the iron: and theſe laſt *vortices* are not broken, becauſe the part of the iron which is rounded or almoſt cylindrical, turning upon its centre or length, does not ſhock the encompaſſed *vortices* in a manner fit to break them. All this is done, as it were in an inſtant, when the iron and flint ſhock each other in a place void of air, and the ſpark is then hardly viſible and does not continue.

But when we let off the firelock in the open air, the part torn from the iron, by whirling ſtrongly, not only meets and ſhakes ſome little *vortices*, but a great many parts of air, which were

were branched, meet, and conſequently break by their motion many more *vortices*, than the little part of the iron could by itſelf. So that the ſubtile matter of theſe *vortices*, coming to encompaſs the fire and the air, it gives them a ſufficient number of different motions, to repel the other *vortices* ſtrongly. Thus the ſparks muſt be much brighter in the air than *in vacuo:* they muſt alſo continue longer, and have force enough to ſet fire to the gun-powder. And this powder cannot want ſubtile matter to ſet it on fire, what quantity of powder ſoever there is, ſince it is not only the matter of the firſt element, as M. *Deſcartes* believed, but much more that of the ſecond, or of the ſmall broken *vortices*, that produces the extraordinary motion of the fire in the mines. If we reflect upon what happens to the fire, when a great deal of air is driven againſt it, we ſhall not queſtion but that the parts of air are very fit to break a great many of the *vortices* of the ſecond element, and conſequently to determine the ſubtile matter to communicate a part of its motion to the fire. For it is only from this matter, that the fire can draw its force or its motion; ſince it is certain, that a body cannot move but by the action of thoſe which encompaſs or ſhock it. The prodigious effects of the great burning mirrours ſufficiently prove, that the ſubtile matter is the true cauſe of fire. The rays of light croſſing each other at the focus of theſe mirrours, the little *vortices* of the ethereal matter of which theſe rays are compoſed, muſt change their circular motion different ways, and tend to move themſelves all the ſame way, that is, according to the *axis* of the cone of reflected light, and to pierce alſo and ſhake the parts of the body which they meet, and to inflame them.

II.

II. *A deſcription of a new ſort of flood-gate, uſed in the undertaking of the new navigation of the* Seine, *by M.* des Billettes *.

This gate is quite ſingular, and has no relation to any that has been ſeen in any of the other navigations. The principal properties of it are:

1. It ſaves at leaſt a foot of excavation, which is known to be a great advantage in this ſort of works.

2. It is of extraordinary ſtrength, and in a manner invincible.

3. It is not faſtened to the piles, and conſequently independent on the inconveniences of all thoſe that are ſo.

4. A ſingle man can open it with ſo much eaſe and quickneſs, that a mariner, when he is within a hundred toiſes above or below, need only blow a horn, and he finds it open before he gets to it. Thus the navigation is never interrupted, as it uſually happens at other gates, where whole hours are often loſt whilſt they are opening them.

5. If there are any repairs to be made, two men can get it out of the water in $\frac{1}{4}$ of an hour; and as ſoon as it is mended, they replace it in an inſtant.

6. There can be no gathering of ſand or other things, to hinder the opening of it, but what may be taken away in a moment.

7. Tho' it is very ſimple, and more certain than any that has been made till now, it coſts much leſs, and is not ſubject to the accidents of other conſtructions.

It was invented by the late duke of *Rouanez.*

* May 2, 1699.

The conſtruction.

We ſhall here only give a geometrical plan of this gate, with a profile of one of its arms, without entering into the explication of thoſe parts, which are neceſſary to all ſorts of ſluices.

Having therefore ſuppoſed the excavation to be as it ought, and the two floors above and below proper for the emplacement of this gate, whether open or ſhut, with all the conditions requiſite to hinder the water from paſſing underneath, or damaging the piles of the canal, we ſhall content ourſelves with obſerving here, that theſe two floors are in a manner made into one, being joined by a platform common to both, and making part of them.

This platform is here repreſented * by 4 pricked lines, and marked A. B. B. A. It is made of 4 pieces of crooked wood, placed end to end, and making together 48 feet in length, and 18 inches in breadth. Its bend is 2 portions of a circle, which meet in the middle of the canal at the points B. B. the centres of which are about 30 feet below, and a little without its alinement. This canal is here marked by the 2 parallel pricked lines *c. c. c. c.*

Into each of theſe two centres is driven a great ſtake 15 or 16 feet long, and 18 or 20 inches thick, of which there remains out of the ground only the length of about 2 feet $\frac{1}{2}$, rounded into a trunnion of 10 inches diameter. Theſe two ſtakes are the pivots of the gate, and marked P. P. It has ſince been found more proper (inſtead of making this trunnion out of the thickneſs of the wood) to cut the head of the ſtake quite

* Plate II. Fig. 3.

ſmooth,

ſmooth, and place in the centre of it a pin of iron about 4 inches in diameter or a little more.

We ſhall now ſhew in what the gate alone conſiſts.

It has 2 leaves, each of which is compoſed of 2 great arms and 2 ſmall ones.

The great arms marked D. D. D. D. are pieces of wood, 30 feet long, and 10 or 12 inches ſquare, joined and bound together with great ferrels and iron pins at the lower end, and ſeparated at the upper end by a diſtance of 5 or 6 feet. At a little above the diſtance of a foot from the extremity of their union, they are cut in ſuch a manner as to form a round hole, into which the trunnions of the pivots enter. At their ſeparated ends they are mortiſed into 2 curbs of wood, 12 or 13 feet long to 7 inches ſquare, marked E E, which are arched like the platform, and meet in like manner at the points B. B. when the gate is ſhut. And on both ſides they are faſtened to the curbs by mortiſed binders F.

At 10 feet from the curbs, the arms are faſtened by croſs quarters G G; and in like manner alſo at about 8 or 9 feet from the extremity of their lower ends.

At about 6 feet from their lower extremity, there ariſe alſo 2 other ſmaller arms of which the tenons below enter into the mortiſes H; and ſeparating afterwards by a very acute angle, riſe at their other end about 4 feet higher, and are mortiſed into another curb raiſed parallel above the firſt. They are again ſupported by upright poſts, mortiſed into the great arms at the places I.

One of theſe little arms joined to the great one, and at once to the upper and lower curbs, may be ſeen in the profile mentioned afterwards.

As the great arms are faſtened by binders to the lower curbs which reſt on the platform, the ſmall ones are likewiſe faſtened by like binders, to curbs 4 feet higher than theſe, and are likewiſe held by a croſs quarter like that at G.

Theſe upper and lower curbs are connected with upright poſts, and binders mortiſed into each. The place of theſe mortiſes may be ſeen in 12 places of theſe lower curbs.

Againſt theſe curbs are faſtened vertically, on the ſide of their convexity, ſome planks, which make a kind of ſection of a portal: and it is thereby that the water is kept up on the floors above and below. They are allowed to be 6 or 7 feet high or more, according as there is water to ſuſtain in proportion to the piles and *chambords* of the canal.

One of theſe planks is marked in the profile, at * *e e*.

The two upright poſts, which are at the extremity of each curb, bear at the bottom each of them on a roller, or cylinder, 10 inches in diameter, and 7 long. And it is by theſe 4 rollers that the gate turns upon the platform: one of theſe rollers is in the profile marked *j*.

At the bottom of the planks in the fore part, or above the ſtream, there are little boards applied reſting on the *chant*, and faſtened by hinges, ſo that they play like the lid of a box againſt the tringle of wood raiſed and nailed along the whole bend of the platform. So that by this means the water cannot enter under the gate by the intervals of the rollers; and yet when any weeds, ſand, wood, or other ſubſtances ſtop before or above theſe boards, they are let under the gate in a mo-

* Fig. 4.

ment,

ment, by raiſing theſe little boards with a chain or hook.

To open and ſhut the gate eaſily, there is placed on each ſide a vertical windlaſs, or capſtan, near the edge of the canal. About this windlaſs is an iron chain, the two ends of which are faſtened near each other on the curb above, after having made a turn on a roller or pully faſtened to the pile; and by this means one man alone turning the capſtan, by a lever, one way opens the gate, and the other way ſhuts it in a very little time.

Each leaf of the gate, when it is open, places itſelf in an excavation as low as the floors, repreſented by the triangles of pricked lines * A. K. L. This emplacement is called the *étang*, or pond, becauſe the water has no courſe.

To keep the boats from ſtriking againſt the pivots, there are two ſtakes Q. Q. placed before them, which are called *pieux de garde*, or *pieux de defenſe*; that is, guarding ſtakes.

This gate may be made ſimple, and has ſometimes been made ſo; that is, of one leaf only, which bars the whole breadth of the canal. It has only 2 great arms at the extremities, that is, to right and left, and two others ſhorter between them, butting only on a croſs quarter which holds the firſt; and very little different alſo in its conſtruction, wherefore it is not neceſſary to ſpeak of it here, becauſe the former is much better. This ſimple one has but one leaf, which extends over the whole breadth of the canal; it turns alſo on a ſingle pivot, and has need only of one pond to place itſelf when it is open. But tho' this ſingle pivot muſt ſuſtain the whole effort of the water of the canal; whereas the two-leaved gate has 2 pivots, each of which ſuſtains but half this effort,

* Fig. 3.

yet the ſimple one is however very excellent, and may be neceſſary on ſome occaſions, where we can have the pond but on one ſide.

An explanation of the profile of one of the leaves, which muſt be imagined to be elevated on the geometrical plan of the others.

* *a a* D D I R is the aſſemblage of the great and little arm, with one of the upright poſts of the gate, which is marked R.

H is the place of the mortiſe marked in the plan of the gate: I one of the upright poſts which holds the upper and lower arms together.

P is one of the great ſtakes ſerving for pivots to the gate: T is the trunnion.

M is a brace of 2 large pieces joined together to hold the trunnion.

N is one of the ſtakes which hold the brace in notches, and ſerve together with the brace to make the pivot ſtronger againſt the force of the water ſuſtained by the gate.

j is one of the 4 rollers. They are faſtened to the bottom of the upright poſts by blocks and gudgeons.

e e is one of the planks which form the leaf of the gate.

III. *An explanation of ſome ſingular effects which happen to plain glaſſes, ſuch as looking-glaſſes, by M.* de la Hire, *at the obſervatory.*

When we look at an object thro' a plain glaſs, that is well poliſhed on both ſides, as a looking-glaſs, we do not perceive that it is multiplied ſe-

* Fig. 4.

veral

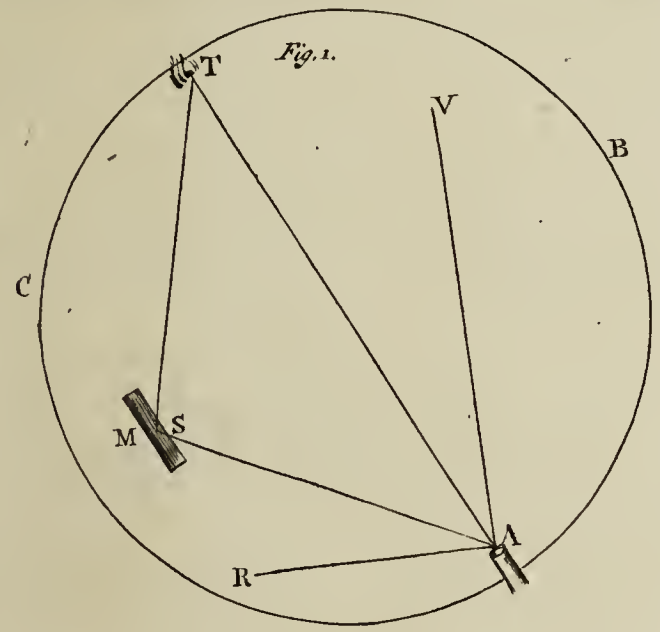
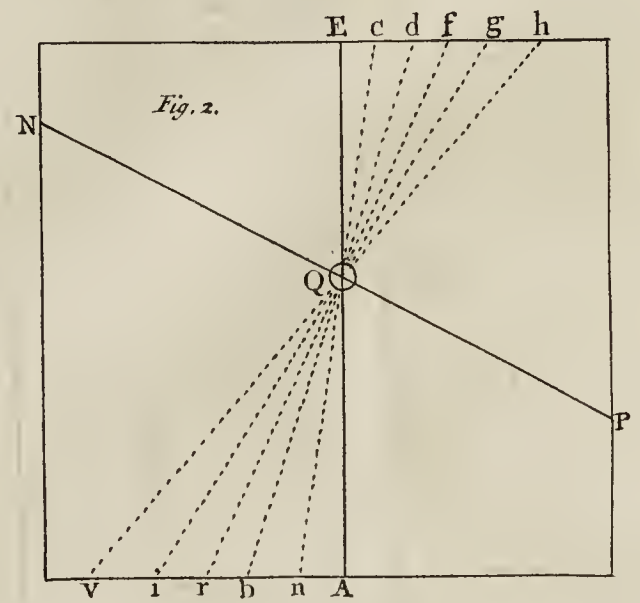
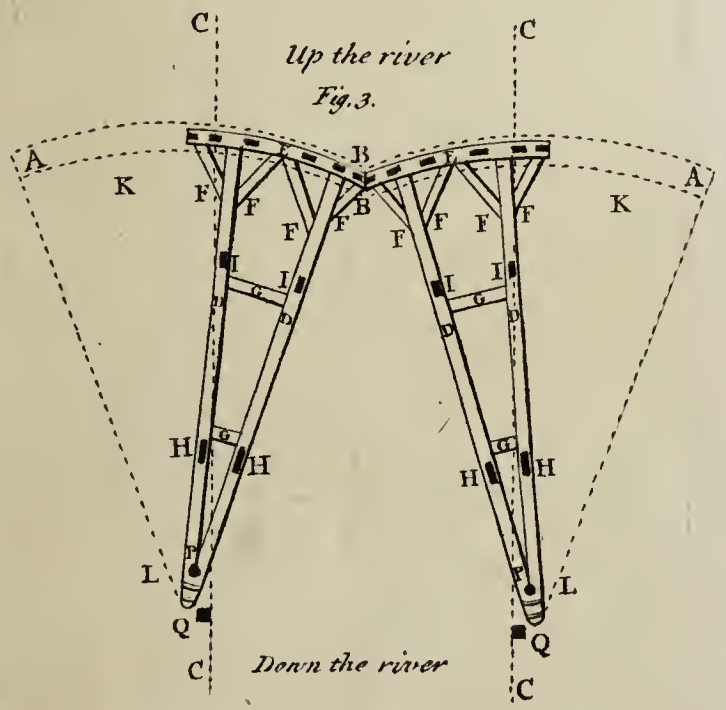


Fig. 1.
T
V
B
C
M
S
A
R


E c d f g h
Fig. 2.
N
Q
P
V i r b n A


C
C
Up the river
Fig. 3.
A
K
F
B
I
G
D
H
L
Q
Down the river

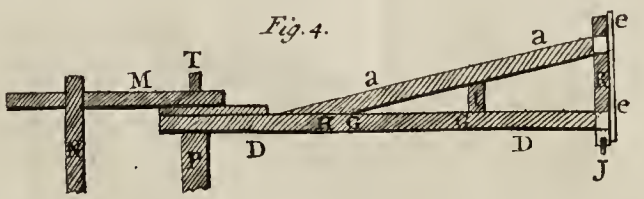
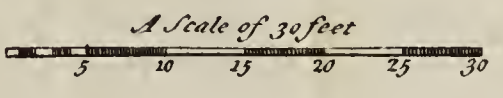


Fig. 4.
M
T
a
e
D
J


A Scale of 30 feet
5
10
15
20
25
30

J. Mynde sc.

veral times: but if it is in darkneſs, and the object is a luminous body as a candle, we may ſee it multiplied at leaſt 4 times; and the multiplied object appears ſo much the more diſtinctly, as the glaſs is more oblique to the line drawn from the eye to the object.

This repetition of the object ſeems ſometimes on one ſide and ſometimes on the other of the luminous body, and theſe appearances diminiſh a little in vivacity, in proportion as they are farther diſtant; and at laſt they become ſo weak as to be no longer perceived. But there is an oblique poſition of the glaſs in which the candle does not ſeem multiplied; and there are ſome glaſſes alſo, thro' which we cannot ſee any multiplication of the object, tho' it is very much inclined.

I firſt obſerve, that this appearance is ſenſible only when the glaſs, tho' plain, is unequal in thickneſs. And as looking-glaſſes generally have their ſurfaces undulated, which comes from the manner of poliſhing them, for they are generally well wrought and ſmoothed, by rubbing them againſt one another; which ſhews the object multiplied in ſome poſitions of the glaſs, ſometimes on one ſide, ſometimes on another, and in ſome poſitions there does not appear a ſenſible, if any, multiplication.

I ſay now, that the moſt diſtinct multiplication of the luminous object is made in the line, which is the ſection or meeting of a plane, which paſſing thro' the eye and thro' the object, is perpendicular to both the ſurfaces of the glaſs together, and that this multiplication is made by the different reflections of the light within the glaſs, and by the refractions in coming in and going out, as I ſhall demonſtrate.

In

In the firſt place, it is eaſily known, that the luminous rays cannot make ſeveral turns and returns both within and without the glaſs, unleſs it be in plain ſurfaces, perpendicular to each ſurface of the glaſs, and which paſs thro' the ray within the glaſs, ſeeing all theſe turns, both in reflection and refraction, are always in a plane which paſſes thro' the perpendicular to the ſurface, which makes the reflection or refraction; and ſince there are two ſurfaces, there will be alſo two perpendiculars; and if theſe perpendiculars are in the ſame plane, all the rays, both refracted and reflected, will be in the ſame plane, and they will make angles with each other more obtuſe, than if they were on different planes, and inclined to each other; which muſt be underſtood only of the rays, which coming from the object may meet the eye.

I ſay now, that if the glaſs was all over of equal thickneſs, or if the ſection of its ſurfaces was two lines parallel to each other on a plane perpendicular to the glaſs which paſſes thro' the eye and the luminous object, this object could not appear ſenſibly multiplied, provided it was at a conſiderable diſtance from the glaſs.

Let * the ſection of the glaſs be E M B D, of which the 2 lines E M, B D are parallel; let the luminous point be C, and the eye O. It is evident, that all the rays, as C A, which coming from the object C, meet on the ſurface of the glaſs E M, are refracted and turn as at A B, as they approach the perpendicular within the glaſs, according to the rules of refraction, and come out afterwards at B O; in ſuch a manner that the angle of incidence C A E is equal to that of the exit D B O, ſeeing the refracted angle E A B is

* Plate III. Fig. 1.

equal

equal to D B A, becauſe of the parallel ſurfaces. Thus the eye being at O, will ſee the luminous point C, by the ray O B parallel to C A.

If the ſurfaces are not parallel, but the ſection E M, G D is two parallel lines, it will be almoſt the ſame thing, ſeeing the whole difference will lie only in the reflected rays, which will not be on one ſurface, by which this demonſtration is not affected.

But for the ſame reaſon, if another ray C F, which may be conſidered as parallel to C A, if the point C is at a conſiderable diſtance from the glaſs, tho' the ſpace A F is large enough, falls on the part A E of the ſurface E M of the glaſs, it will have its refracted ray F G within the glaſs, which will alſo be parallel to A B; and that as G H which ſhall go out of the glaſs, being alſo parallel to B O, will not be able to enter the eye O, unleſs it is very near to B O, which I do not ſuppoſe here. It would be the ſame with another ray, which ſhould meet the ſurface of the glaſs on the other ſide of A towards M. But if one part of the ray F G is reflected at I by the laws of catoptricks, and if another part of the ſame ray G I is reflected at I K, which will alſo be parallel to A B, becauſe of the parallel ſurfaces E M, B D, goes out of the glaſs at K, being refracted at K L, it is as evident, that this ray K L will either meet the eye at O, or not at all; and if it does meet it, it will not make a different image from that of the ray B O, ſeeing it enters the pupil with the ſame direction as B O, wherefore in this caſe the object will not appear multiplied. But if we ſuppoſe, that C F makes the angle C F E much more obtuſe than the angle C A E, then it may be that the ray K L, which will alſo make the angle D K L equal to C F E, will meet the eye in O,

under another direction than B O; which will form an image of the luminous point, different from that made by the ray B O.

But this ſecond image formed by K L, will be a little weak, becauſe the greateſt part of the rays will be loſt in coming out of the glaſs at G and at I.

It muſt be obſerved, that if the glaſs is not very thick, or the object not far enough off, the ray K L will be very near BO, and conſequently the image formed by K L will be ſo much the more eaſily confounded, as it is more weak, with that which is formed by B O; and that ſo the object will not appear ſenſibly multiplied.

It is not neceſſary to demonſtrate that the ray, which coming from the point C would fall upon the part A M of the ſurface of the glaſs, would make a more obtuſe angle than C A M, and could never meet the eye after one or more reflections, ſeeing it would always ſeparate more and more from BO.

Now * if the glaſs is ſuppoſed to be of unequal thickneſs, and to be thicker towards the object C than towards the eye O; 1. The ray C A B O, which will come to the eye after two refractions, will come to the line BO, which being prolonged will meet the incident ray C A; for the ray A B refracted within the glaſs, being more perpendicular on B D than A M, the ray alſo O B will make with A B a more obtuſe angle than C A with the ſame A B, wherefore O B being prolonged will meet C A towards the object C. And if the bigneſs of the glaſs does not hinder the rays immediately from the object C towards the eye O, we ſhall ſee the object C ſim-

* Fig 2.

ply

ply out of the glaſs, and on the ſide the object thro' the glaſs by the ray O B.

2. There will be other rays as C R, which coming from the object C, after a refraction R N and two reflections N M, M P, and a refraction P O, may come to the eye O. For ſuppoſing C R as parallel to C A, R N will be alſo parallel to A B. But the ſurface B D being able to meet the ſurface A M in D, the ray N M will be more perpendicular to the ſurface A M than the ray N R, which does not merit demonſtration; and the ray M P making the angle PMD equal to the angle NMA, the angle MPD will be leſs obtuſe than the angle R N D or A B D: wherefore the refracted ray P O making the angle D P O greater than the angle D B O, it may meet B O at the point O, which is the eye.

We ſhall find alſo other incident rays at C R, which meeting the ſurface A D beyond R towards D and after two refractions both in coming in and going out, and 4 or 6 or 8 reflections within the glaſs, may fall on the ſame point O, which will ſhew the object as many times multiplied as there are different rays, which come to the eye O. And theſe rays will come to the eye in the ſame manner as if the luminous object was placed in the rays O P, prolonged to a diſtance from the point O equal to the ſum of all the rays O P, P N, MN, N R, R C; for they will enter the eye with the ſame direction, as if they came effectively from a point placed at this diſtance.

It is known by what I have juſt demonſtrated, that the laſt images of the object, which are thoſe that come to the eye after a greater number of reflections, and conſequently after a longer way, will appear ſmaller than thoſe which are nearer the firſt O B, becauſe coming as it were from a

greater diſtance, the rays enter the eye more parallel to each other, which makes the object ſeem farther off; and the faintneſs of the image of the objects moſt diſtant from O B, contributes alſo very much to make them appear ſmaller.

It is eaſy to ſee, that the rays which come to the eye, after a greater number of reflections within the glaſs, muſt appear much fainter than thoſe which come thither after a leſs number: ſeeing at each meeting which the ray makes with one of the inner ſurfaces, the greateſt part of the rays comes out of the glaſs; and as very few are reflected, ſo the laſt that come out and make only part of thoſe which are reflected, muſt be very faint: it is for this reaſon alſo, that if the body was not luminous, and if it was not in the dark, we ſhould not perceive any multiplication.

It muſt now be demonſtrated, why there appears a greater diſtance between each multiplied object, when the glaſs is more oblique to the ray which goes from the object to the eye, than when it is leſs oblique.

Let * there be the ray CABO, which meets the eye in O after two refractions, one in coming in, and the other in going out of the glaſs; and another ray C R N M P O, which comes alſo to the eye in O after two refractions and two reflections in the glaſs. Let there be alſo another ray Kφ, of another luminous point K, more inclined to the glaſs than CA, and paſſing thro' Kα Bω, and another Kϱ N$\mu\pi\omega$, which coming alſo from K after two refractions and two reflections, meets Bω in ω where the eye would be; I ſay, that the angle B$\omega\pi$ is greater than the angle BOP, and conſequently the two images which come to the eye by the rays OB, OP, and their ordinates, will appear leſs ſe-

* Fig. 3.

parated

parated from each other, than thoſe which ſhall be formed by the rays ωB, $\omega\pi$.

The ray αB in the glaſs makes with AB the the angle αBA, ſmaller than the angle ωBO formed by the ſame refracted rays in coming out of the glaſs, which is known by dioptricks; for the rays as BO which are the refracted of the incidents AB, approaching much quicker to the ſurface of the glaſs BD, than the incidents in the ſame glaſs do to the ſame ſurface HB. But the line ρN being as a parallel to αB, becauſe we ſuppoſe the rays $K\alpha$, $K\rho$ as parallels, it follows that the angle ρNR will be equal to the angle $MN\mu$; and conſequently the difference of the angles DMP, $D\mu\pi$ will be alſo equal to the angle αBA.

But the rays MP, $\mu\pi$, being more perpendicular to the ſurface BD than AB and αB, the refracted rays PO and $\pi\omega$ will make between them a ſmaller angle that $OB\omega$, and if we draw πI parallel to PO, the angle $I\pi\omega$ will be equal to that which is contained by the rays PO and $\pi\omega$, which will be ſmaller than the angle $OB\omega$; which is plain from the reaſon juſt given.

Now becauſe the external angles are equal to the two internal oppoſite ones in the triangles, the angle $D\pi\omega$ *minus* the angle $DB\omega$ will be equal to the angle $\pi\omega B$; and the angle DPO *minus* the angle DBO will be equal to the angle POB: but the difference between the angle $D\pi\omega$ and DPO, or its equal $D\pi I$, which is the angle $I\pi\omega$ was demonſtrated above to be ſmaller than the angle $OB\omega$: wherefore if from the angle $D\pi\omega$ we take away the angle $DB\omega$; and if from the ſame angle $D\pi\omega$ *plus* the angle $\omega\pi I$, we take away the angle $DB\omega$, and the ωBO, which is greater than $\omega\pi I$, as has been demonſtrated, it follows that the firſt remainder is greater than the ſecond, ſeeing in the

ſecond there is more taken away than added; but the firſt remainder is equal to the angle $\pi\omega B$, and the ſecond equal to POB; therefore the angle $\pi\omega B$ is greater than the angle POB, Q. E. D.

It will be the ſame with the other angles made as $\pi\omega B$ by incident rays which will fall towards D.

Experience confirms what I have juſt demonſtrated of the images which appear by the rays OB, OP, OQ, *&c.* which are formed by the incident rays CA, CR, CS, and thoſe which are of the ſame ordinance, or which coming from the ſame point of the luminous object, can enter the pupil: for if a paper is applied to the ſurface of the glaſs AD, and it is drawn forward from D towards A, when the paper ſhall cover the place S of the incident ray CS, the luminous image in Q will diſappear. Afterwards when it is come to R, the light in P will diſappear in its turn, and ſo on. And it will be obſerved, that no regard is to be had to the image of the paper, which is ſeen doubled becauſe of the refraction, which is not ſeen out of the darkneſs; but it is a particular caſe which I ſhall explain afterwards.

The diſtances between the images BPQ appear almoſt equal among themſelves; for theſe images are formed by reflected rays, which ſurpaſs one another by a ſeries of two, and the incident rays CA, CR, CS, which form them, are very near to each other: but this appearance can only be when the ſurfaces of the glaſs are almoſt planes; for if one or both of them are curves, the diſtances between the images will appear very unequal; which is eaſy to know by the diſpoſition of the rays which can come to the eye.

I have already demonſtrated, that the images appear more faint in proportion as they are farther removed from thoſe which are neareſt the luminous

minous object, by the quantity of rays which escape out of the glass; and I have said also, that they appear smaller, because they come to the eye as if they were farther off: but I demonstrate at present, that they appear larger in proportion as they are farther separated from the luminous object, which must always be understood of the images multiplied in the plane, which passing thro' the object and thro' the eye is perpendicular to the two surfaces of the glass.

We shall explain this *phenomenon*, as we did the preceding. For the rays, which come to the eye * O from the two points CK, which are on the two sides of the luminous body, after two refractions only, are more inclined to the surface of the glass which is turned towards the eye, than those which come thither after two refractions and two reflections within the glass; which appears by the figure, and by what has been already demonstrated. But since the rays more inclined to the surface BD make angles much more acute in proportion, as they approach to the surface of the glass, than those of inclination are, because the refractions follow the proportion of the sines of the angles of inclination; it follows that the rays OB, O3 will make an angle BO3 much more acute in proportion than the rays OP, Oπ, which comprehend the angle POπ. For the ray O3 must be more inclined with regard to its incident ray, than the ray Oπ with regard to its incident: thus the angle BO3 will be more acute in proportion to the length of the ray which goes to the object by the point B, and by its turns within the glass, than the angle POπ in proportion to the length of the ray which goes to the object by the ray OP and by all its turns. Therefore, lastly,

* Fig. 4.

the image represented to the eye by the angle POπ, will appear larger than that which is represented by the angle BOβ, in proportion to the height of these images. For the height of the image of the body, which I have supposed to be a candle, must diminish only in proportion to the ways which the rays take both within and without the glass to come to the eye, seeing the section of the glass, which determines the greatest inclination of its surfaces, is supposed horizontal.

It must be observed, that in all which is here explained of the rays that come from the points of an object as CK, I speak only of the principal rays; tho' we must always conceive an ordinance of the rays of these same points which come all to the eye to enter the aperture of the pupil, which make a sort of cones or pencils as some call them, which assemble on the *retina* to make the picture of the object; for it is demonstrated in dioptricks, that the point, where they assemble after their refractions in the eye, is always upon one of these rays called the principal.

* As for the paper which hides the multiplied images as fast as it advances, as I have said before, it is certain that we could not see its edge M N multiplied, if it was not the luminous body that makes it appear. For if the paper MN begins to hide the incident rays VT, which shew the breadth of the image D of the candle, the edge of the paper, which will appear at OP will hide also a part of this image; but if the same edge of the paper MN hides the greatest part of the rays SR, which shew the image Q, it is evident that only a small part of this image will be seen as at Q, where we may perceive the image of the edge of

* Fig. 5.

the

the paper that hides it, which one could not perceive without the light which appears at Q.

I am now to demonſtrate how the multiplication of the object is made, when the angle of the ſection of the glaſs is turned towards the luminous object; but after what has been ſaid above, the figure may be ſufficient to explain what ought to happen.

Let * the ſection of the glaſs therefore be EDB, of which let the concurrence of the ſurfaces in D be turned towards the object C, and let the eye be placed oppoſite towards O. Firſt let the incident ray be CA, which being refracted in the glaſs at AB, in going into it, and refracted again at BO in coming out of it, comes to the eye O; I ſay, that if the glaſs ſhould be cut in B, and the part BD taken away, we could not then ſee the object C.

For the ray AB within the glaſs, being more inclined on the ſurface DB than on DAE, the angle ABO will be leſs obtuſe than the angle BAC; wherefore the ray CA will concur with the ray OB towards O, and conſequently there can be no direct ray which comes from the object C to the eye O.

But it is as evident, that all the rays which coming from the luminous object C, and meeting the ſurface of the glaſs towards E, can never come to the eye, which we ſuppoſe placed in the ray BO, after two or more reflections in the glaſs, ſeeing they will be always more and more inclined to the ſurface DB, and conſequently they will come out of the glaſs in parting from the ray BO. The rays therefore which fall upon AD towards the concurrence of the ſurfaces of the glaſs, will be

* Fig. 6.

thoſe

thoſe which make the multiplication of the object.

For in the ſecond place, let the incident ray be CR, which coming from the luminous object C as parallel to CA, meets the ſurface DA in R, the refracted ray RN will therefore be alſo as parallel to the refracted ray AB; but the reflected ray NM will be more inclined to the ſurface DA than NR, and placed counter to the quantity of the angle P of the two ſurfaces of the glaſs, as I have demonſtrated in the firſt caſe: wherefore the reflected ray MP will make the angle EMP more acute then the angle ERN or its equal EAB; therefore the ray MP in coming out of the glaſs by PO will make the angle XPO more acute than XBO; and conſequently PO may meet BO at the point O, where the eye is in the ray BO.

It will be the ſame thing with the other rays which coming alſo from the object, may meet the eye after two refractions of 4 or 6 reflections within the glaſs, which it is not worth the while to explain more at length.

If we draw a paper on the ſurface of the glaſs from the part E towards A, we ſhall ſee that the ſtrongeſt image formed by the ray CA will diſappear firſt, and the reſt afterwards, as faſt as the edge of the paper ſhall advance towards D; and we ſhall ſee the image alſo of the paper multiplied as in the preceding caſe. For in the ſame figure, if the edge of the paper MN covers the greateſt part of the ſpace SR, which comprehends the rays which form the image Q, alſo the image XY of the edge of the paper will appear to hide the greateſt part of the image Q of the luminous body. And if the ſame edge of the paper MN in the ſame poſition covers alſo a great part of VT, by which the rays enter that form the

image

image D, we ſhall ſee alſo the edge of the paper repreſented in OP, which will cover a part of the image P, which is eaſy to underſtand.

By means of this multiplication of the objects, we may know the different thickneſſes of a piece of looking-glaſs which is uſed in working the great teleſcope glaſſes, and in a much more ſure and juſt manner, than by all the meaſures that can be taken for it.

But to make this explanation more complete, we muſt ſtill examine how the multiplication of the object is made, when it is placed on the ſame ſide with the eye, with regard to the ſame ſurface of the glaſs.

Every body knows, that when we look at a candle placed near a looking-glaſs which has been tinned, the eye being alſo near the glaſs, we ſee the image of the candle multiplied ſeveral times. It is very eaſy to ſee the reaſon of it; for the anterior ſurface of the glaſs reflects the firſt image, and the other ſends back the ſecond, after two refractions of the rays in coming in and going out of the glaſs, and a reflection on the tin. But what is more difficult to be known, is the reaſon why there appear more than two images. This experiment and its explanation ſerve no doubt for a proof of what I have already advanced concerning the luminous rays which are reflected within the glaſs in meeting its ſurfaces, tho' they are not tinned. It is eaſy to ſee, that if the glaſs is tinned, all the rays, which ſhall enter the glaſs, cannot go out of it, and that they muſt all be reflected, and conſequently the image which ſhall be formed by this reflection and after two refractions, will be much more vivid than that which is made by the reflection alone on the anterior ſurface, becauſe in this the greateſt part of the rays having

penetrated within the glaſs, cannot meet the pupil of the eye in going out, if the reflected only have met it, and eſpecially if the glaſs has a little thickneſs. But as the ſame thing happens to a glaſs which is not tinned, as to one that is, and as ſome particular obſervations may be made on that which is not, that are not found in that which is, I ſhall here explain only what happens to a glaſs which is not tinned.

As the different images of the ſame object may be formed by rays which have different inclinations to the eye, theſe rays muſt neceſſarily come from different parts of the ſurface of the glaſs, which cannot be without the firſt incident rays of one point being different from each other, we ſuppoſe the ſurfaces of the glaſs to be planes, and parallel to one another; and theſe different inclinations of the rays cannot meet the pupil, which is as a point, till after ſeveral reflections within the glaſs. But if theſe rays which come out of the glaſs, are, as it were, parallel to each other, which happens when the object is far enough from the eye, and the glaſs is of equal thickneſs, or when they are on the ſame plane, which being equally inclined on each ſide to the ſurfaces of the glaſs, and paſſing thro' the eye and the object, makes parallel ſections, it is evident, as I have already demonſtrated, that the rays of the ſame point having ſuffered ſeveral reflections within the glaſs, cannot meet the eye; there will then in this caſe be only one image formed by theſe ſorts of rays. But if in the ſame caſe of parallel ſurfaces, the object is near the eye and the glaſs, there may fall on the ſurface of the glaſs ſome rays from one and the ſame point of the object differently inclined to form ſeveral images, as is ſhewn by experience.

If the glaſs is of unequal thickneſs, and the right lines which are the ſection of the glaſs by a plane perpendicular to its ſurfaces paſſing thro' the eye and the object, concur on both ſides, we ſhall ſee ſeveral images of the object, at leaſt if it is luminous, and in the night ; ſince at whatſoever diſtance from the eye the object is placed, there will be found ſeveral rays which coming as parallels from the ſame point of the object, and having ſuffered ſeveral reflections within the glaſs, will come out of it with different inclinations, which will form different images, which has no need to be explained after what I have ſaid already; and theſe images will be found ſometimes on one ſide, ſometimes on the other, with regard to that which is made by one ſingle reflection, according as the angle of inclination of the glaſs ſhall be either towards the eye or towards the object.

We may alſo by this means know with a very great exactneſs, whether the glaſſes poliſhed on both ſides are of the ſame thickneſs, tho' in a very ſmall ſpace ; which it would not be poſſible to obſerve any other way. For if we obſerve on the ſurface of a glaſs the image of a bright line in the dark, or of a black line in the light, and if it is at a pretty conſiderable diſtance, and if the eye and the object are on the ſame ſide of the ſurface, and alſo if in turning the piece of glaſs different ways, we ſee this image only ſingle, in one ſingle poſition, we may be ſure that the piece of glaſs is of equal thickneſs according to the diſpoſition of the bright or dark line, and that it is of unequal thickneſs every other way, the greateſt angle of which will be in the ſection, which ſhall cut the image of this line perpendicularly. Alſo in every other poſition of the glaſs but that where the image of the line is ſingle,

it will be ſeen double; and they will appear farther diſtant from each other, when they ſhall cut at right angles the poſition where it appeared ſingle.

It will alſo be obſerved, that one of the two images of the line appears much more vivid than the other, and that it is this which is made by reflection, becauſe there are more rays that come to the eye.

It will not therefore be difficult to know by this means, what place of the glaſs will be the thickeſt. For if the fainteſt image appears towards the object, I ſay, that the part of the glaſs which is turned towards the object is the thinneſt; and if the fainteſt image is towards the eye, that part of the glaſs will alſo be the thinneſt.

For if the object is * B, and the eye O, and a luminous ray BD meeting the ſurface of the glaſs HE in D is reflected towards the eye at O, it is eaſy to ſee, that if another incident ray coming alſo from the point B, and as it were parallel to BD, meeting the ſurface of the glaſs in F, is on either ſide of D, and penetrating on the inſide to K, meets the other ſurface KI, which concurs towards the firſt in M, on the ſide of the object, the greateſt part of this refracted ray FK will go out of the glaſs; and a very ſmall part only being reflected in K, will return towards the ſurface HE in H, or being refracted in HL, it can never meet the eye in O. For the ray KH, being more inclined to the ſurface HE than the ray KF, alſo the ray HL will be more inclined to the ſame ſurface than the incident ray FB; and conſequently this ray HL, which parts more and more from the ray DO, can never meet the point O,

* Fig. 7.

if

Fig. 1.

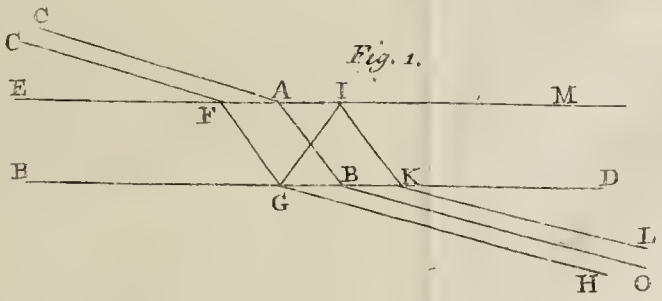


Fig. 2.

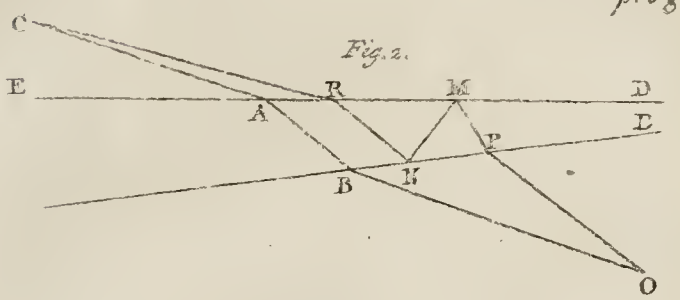


Fig. 3.

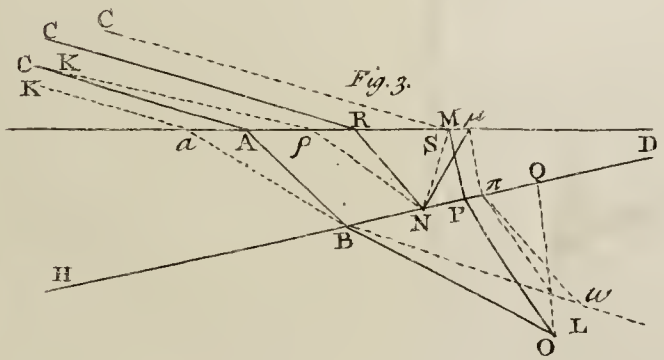


Fig. 4.

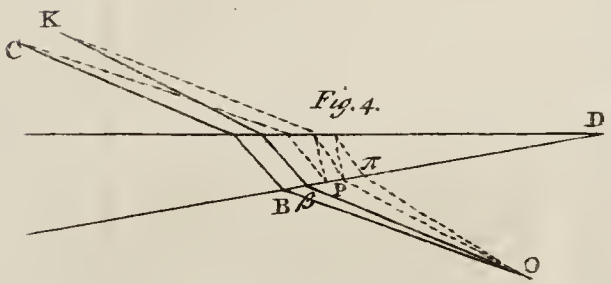


Fig. 5.

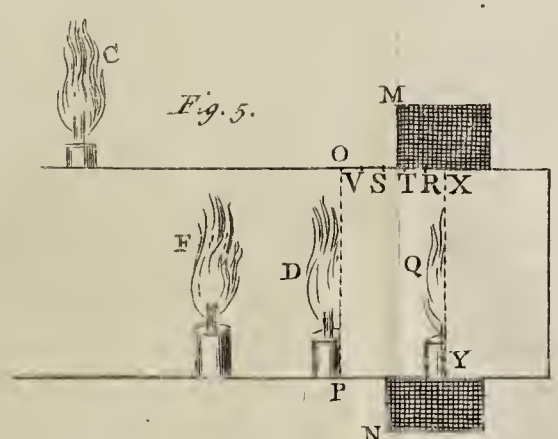


Fig. 6.

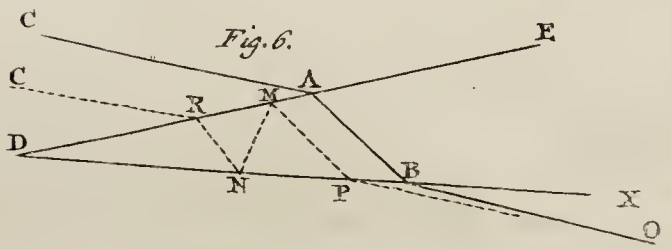
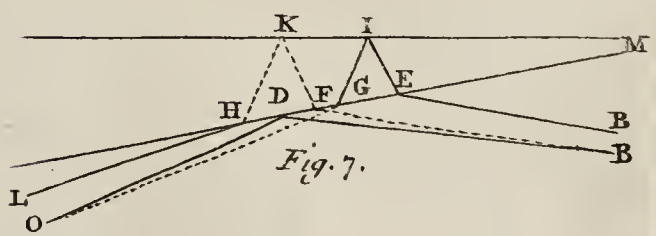


Fig. 7.



if the point H is farther from the top of the angle M than the point D.

It will not be the ſame with another ray as BE, which having ſuffered a refraction in E, a reflection in I, and a ſecond refraction in G, comes out at the point G between D and M; for this ray refracted in G may meet the eye in O, becauſe the inclination HGO is always greater than BEM, or BDM, or ODH, as has juſt been demonſtrated; wherefore the fainteſt image which is formed by the ray OG, which has ſuffered two refractions and one reflection, will appear in G towards the part M, which is the thinneſt of the glaſs.

The demonſtration will be the ſame, if the thinneſt part of the glaſs is turned towards the eye.

IV. *A commodious way of ſubſtituting the action of fire, inſtead of the force of men and horſes to move machines, by M.* Amontons *.

Experiment I. *Of the rarefaction of the air by the heat of boiling water.*

We † immerged in a kettle full of water, the balls of three glaſs tubes ACD, ACD, ACD, of equal length, each open at A, bent at C, and ending in a ball D; the capacities of the balls were to each other as the numbers 1, 2, 3, as well as thoſe of the tubes AB, which beſides were pretty narrow, the mean one being but half a line diameter within; there was in each glaſs ſome quickſilver from the entrance E of the balls up to B, where the quickſilver was three inches higher than at E, becauſe the air of which the balls were

* June 20, 1699. † Plate IV. Fig. 1.

full,

full, having found no out-let when the quickſilver was poured in by the apertures A, ſuſtained it by its ſpring, and hindered it from going down to the level of that of the entrance of the balls.

The whole was ſet on the fire, and the quickſilver at B roſe equally in equal times in all the three glaſſes; ſo that when the water began to ſimper, it was 9 inches higher than B, and 9 inches 10 lines, when it boiled out, after which it entirely ceaſed to riſe. From this experiment it follows:

1. That the heat of boiling water has bounds which it cannot paſs.

2. That unequal maſſes of air increaſe equally the force of their ſpring by equal degrees of heat, and on the contrary.

3. That the heat of boiling waterdoes not increaſe the force of the ſpring of the air any more than to make it ſuſtain the weight of about 10 inches in height of quickſilver, or 11 feet 8 inches of water more than the weight of the atmoſphere *. For the quantity by which the air diminiſhes its ſpring in dilating itſelf to replace the quickſilver which riſes from B to F, equals almoſt the two lines which are wanting to the 10 inches in this experiment.

4. That if the air has the liberty of extending itſelf, being preſſed only by the weight of the atmoſphere, it will not increaſe its bulk by the heat of the boiling water by any more than about $\frac{1}{3}$ of its maſs; for according to M. *Mariotte*'s experiments, the air making an *equilibrium* by its ſpring to weights proportioned to the bulks, to which theſe weights are reduced by their preſſure, and the bulks being to each other in an inverted

* *The weight of water is ſuppoſed to be to that of quickſilver as* 1 *to* 14.

ratio

ratio of theſe weights, if the height of the quickſilver is ſuppoſed at 41 inches, as indeed it may be in this experiment, and if the bulk of air is expreſſed by the number 3; when the height of the quickſilver ſhall be no more than 31 inches, the bulk will be $3\frac{10}{31}$.

But as the weight of the atmoſphere is here ſuppoſed equal to 31 inches of quickſilver, which it really is not, this weight being hardly equal to more than 28 inches of quickſilver, the fraction ought to be reckoned as an integer; becauſe the air loſing the force of its ſpring leſs when it is but little loaded, than when it is more, it muſt not augment its bulk ſo much to reduce itſelf from the preſſure of 41 inches to that of 31 inches, as it muſt in reducing itſelf from the preſſure of 38 inches to that of 28 inches.

5. That if the air, being rarified by the heat of the boiling water, has not the liberty of augmenting its bulk ſo far as to be $\frac{1}{5}$ bigger, the force of its ſpring will always be equivalent to a greater weight than that of the atmoſphere, and this weight will always be to that of the atmoſphere in an inverted *ratio* to that of the bulks; and if the bulk of this air is expreſſed, for example, by the number 7, and the height of the quickſilver which reſiſts the force of its ſpring is 41 inches; when this bulk ſhall be augmented by $\frac{1}{7}$, that is, ſhall be expreſſed by the number 8, the force of the ſpring of the air will be equivalent to $35\frac{7}{8}$ of quickſilver, and what it has loſt will be equivalent only to 5 inches $\frac{1}{8}$ of quickſilver, or 5 feet 11 inches $\frac{3}{4}$ of water, and only 5 feet 6 inches, 6 lines, the calculation being made upon 38 inches, inſtead of 41 inches.

Experiment

Experiment II.

Another time the thermometer being almoſt at temperate; we immerged in cold water the balls of the 3 tubes of the preceding experiment, and the quickſilver fell but to about a line below B, in the glaſs which had the biggeſt ball, to two lines in the next, and to three in the leaſt; after which it left falling entirely in all the three glaſſes; they were afterwards taken out of the water, and the quickſilver continued to fall about a line in the glaſs with the ſmalleſt ball, two lines in the next, and about three lines in the glaſs with the biggeſt ball; ſo that the quickſilver remained for a time in all the three glaſſes about 4 lines lower than B, and roſe again by degrees as faſt as the balls dried.

This experiment being conformable to another which I made 12 years ago in *Auguſt*, when the heats are very great, with the zymoſimeter; the ball of which was immerged in cold water, and yet the air did not diminiſh any more the force of its ſpring, it follows:

1. That the air immerged in the water diminiſhes the force of its ſpring only in ſuſtaining but one line in height of quickſilver leſs than the atmoſphere.

2. That the air diminiſhes its ſpring by the coldneſs of the water in proportion to its bulk; but that the greateſt loſe leſs than the ſmalleſt.

3. That the water, which is ready to evaporate, diminiſhes the force of the ſpring of the air more than when it is in a ſufficient quantity to encompaſs it on all ſides, which is confirmed by this other experiment of the zymoſimeter: for having immerged the ball in the ſpirit of wine, the ſpring of the air diminiſhed and ſuſtained

the

the weight of 4 ounces in height of water leſs than the weight of the atmoſphere; being withdrawn from the ſpirit of wine, it ſtill diminiſhed ſo as to ſuſtain 5 inches of water leſs, which made in all 9 inches of water leſs than the weight of the atmoſphere; being put again into the ſpirit of wine, the ſpring of the air increaſed the 5 inches which it had loſt out of it; and being put again out of the ſpirit of wine, it loſt the 5 inches again. This experiment was made at the ſame time of the year with that mentioned above, that is, during the heats of ſummer.

4. That this ſecond diminution of the force of the ſpring of the air is made alſo in proportion to its bulk; and that it is greater in the biggeſt, and ſmaller in the leaſt.

Experiment III.

We cauſed a cube to be conſtructed of tin * ABCD, exactly cloſe on all ſides, and divided equally into two, by the ſeparation EF; the lower part EBCF has no communication with the upper AEFD; but by the tube GH, incloſed in a bigger IL, ſhut at L, and ſtopped and ſoldered at I, to the upper part of the cube. MN is another tube, which penetrates into the upper part, and reaches near the bottom EF, and is ſoldered to this part in O, and ſtopped and ſoldered by its extremity N to a little bucket or reſervoir P; there is alſo towards A, a little cock to give air to the upper part. This cock being opened, we poured ſome water into the little bucket P, this water deſcended by the canal NM, into the upper part of the cube; when this part of it was quite full, we ſhut the cock, and immerged for 6 ſeconds the lower part of the cube in boiling wa-

* Plate IV. Fig 2.

ter, and a conſiderable part of the water contained in the upper part of the cube being driven by the force of the ſpring of the air, roſe with precipitation into the bucket P. At the end of 6 ſeconds, having taken it out of the boiling water, the water of the bucket began to go down again; but in the ſpace of 300 ſeconds, it was not yet reduced to the ſtate in which it was before. We afterwards put this lower part into cold water to finiſh the reducing of the air to its firſt bulk, after which we put it into boiling water during 6 ſeconds more, and the water roſe again, as before, into the bucket P; after which it was immerged into cold water, and the air reſumed its former bulk in 18 or 20 ſeconds, which was repeated ſeveral times; and very near the ſame thing happened always, whether this lower part of the cube was always kept during 18 or 20 ſeconds in cold water, or whether after having been dipped in it, it was taken into the air. It follows from this experiment:

1. That the hard bodies which are not very thick, as tin, receive very readily the heat of the boiling water.

2. That it is not always the cold alone of the *medium* that deſtroys the action of the heat, ſeeing by the preceding experiments, the air and the water are of almoſt the ſame temperature.

3. That theſe thin bodies are three or four times as long in loſing in the cold water the heat received in the boiling water, as they were in receiving it.

I call that water cold, which is almoſt of equal temperature with the air.

Experiment IV.

In the preceding experiment, the tubes NMGH were but a foot in height, but afterwards they were

were prolonged to eight; and having repeated the ſame experiments, they produced the ſame effect again, excepting that the water did not riſe in altogether ſo large a quantity, which muſt neceſſarily happen, becauſe of the greater height or load of water, which by its weight oppoſed the dilatation of the bulk of air incloſed in the lower part of the cube.

After this, we put the part BC upon burning coals, which made the water riſe into the bucket P, juſt as the boiling water had done, but it did not riſe ſo quickly, becauſe the heat was immediately applied only to the bottom BC, whereas in the boiling water it was immediately applied to the four ſides BE, BF, FC, CE, which made together a double ſurface BC. We could not therefore well obſerve the time which the water took in riſing into the bucket P, becauſe we were not careful enough, that the ſolder ſhould not melt, which happened at laſt, but the water was then in the bucket, at leaſt as high as it had been by the effect of the boiling water, and would have riſen higher without it.

It follows from this experiment, that we may, by the heat of fire applied immediately to the capacity which incloſes the air, increaſe the force of its ſpring much more conſiderably than by boiling water, provided what incloſes the air is able to reſiſt the action of the fire; and that effect of it is ſo much more quick, as the action is made in a greater extent.

Experiment V.

Five men were applied to the motion of a machine, who in puſhing by the levers to which horſes are uſed to be faſtened, employed their whole force to make it move.

Suppoſing the force of each man to be 200 *lb.* the total is 1000 *lb.*

Horſes were afterwards put to work at this machine, they laboured for three months; and tho' 4 horſes were put to at a time, and they were relieved every three hours, and afterwards every hour and half, ſo that each horſe laboured but 6 hours in 24, and went all this time no more than about 6 leagues, yet they were not able to undergo this fatigue, and died under it.

From this experiment it follows, that to continue a like labour night and day, there was need of 16 horſes; and that we cannot reckon that a a horſe ſupplies the place of a continual power of 60 *lb.* making one league an hour.

Experiment VI.

The workmen who poliſh glaſſes, to preſs their poliſhers make uſe of an arrow or bow of wood, one of the ends of which being rounded reſts upon the middle of the poliſher; and the other, which is an iron point, preſſes againſt a ſtrong oaken plank reſted upon their work: the ſimplicity of this machine cauſes the whole force of the workman to be ſolely employed in diſpatching his work. The poliſhers moſt commonly uſed are of three bigneſſes, and require three different degrees of ſtrength, of which the mean quantity is about 25 *lb.* now theſe operators uſually begin their work at 5 in the morning, and finiſh it at 7 in the evening, taking two hours at three times for their meals; ſo that in 24 hours they regularly work 12, interrupted every 3 hours by their meals; the flight of their arrow, or the way that their poliſher makes every time they puſh it, or draw it back, is uſually a foot and half, and the time employed at each flight half a ſecond; but as

they

they ſtop from time to time to look at their work, and to bruſh their poliſhers, and beſides as they employ ſome time in turning their glaſſes, about $\frac{1}{6}$ of the time is to be ſubſtracted from their work, ſo that out of the 12 hours we can hardly reckon above 10, during which their labour is equivalent to the continual elevation of a burthen of 25 *lb.* at 3 feet in a ſecond.

It follows from this experiment: that to keep up a like labour for 24 hours would require two men; and therefore that a ſingle man is in lieu only of a continual weight of 12 *lb.* $\frac{1}{2}$, making $\frac{3}{4}$ of a league an hour; that is, about $\frac{1}{6}$ part of the labour of a horſe.

The following experiments were made with poliſhers of different bigneſſes, preſſed by arrows of different forces.

A poliſher 6 inches in ſurface preſſed by 28 *lb.* was drawn by 23 in its fort, that is, when the arrow was plumb, or in the middle of its flight, and by 20 *lb.* in its feeble.

The ſame preſſed by 30 *lb.* was drawn by 26 *lb.* in its feeble.

Another poliſher 11 inches in ſurface, preſſed by 28 *lb.* was drawn by 25 *lb.* in its fort, and by 23 *lb.* in its feeble.

Another poliſher 24 inches in ſurface, preſſed by 28 *lb.* was drawn by 25 *lb.* in its fort, and by 23 *lb.* in its feeble.

The ſame loaded with 30 *lb.* was drawn by 28 *lb.* in its fort, and by 25 *lb.* in its feeble.

From theſe experiments we may obſerve, by the by, that it is an error to think, that the frictions in machines increaſe or diminiſh in proportion, as the parts which rub are more or leſs extended; and that the wheels, for example of a mill turn ſo much the more eaſily as the gudgeons

are ſhorter, which beſides is a faulty conſtruction, becauſe they continually eat the boxes, in which they turn. But that theſe frictions augment or diminiſh in proportion to the burthens which are moved, and to the *ratio* of the length of the levers, which ſerve to move them, to the length of thoſe by which they are ſupported.

Problem.

Any number of equal weights being given * B, G, H, I, L, on the ſemicircumference of a vertical wheel, from the plumb line which paſſes thro' the centre of the wheel, to find the reſiſting force, which being applied to the circumference of the wheel, ſhall make an *equilibrium* to the given weights.

The ſolution demonſtrated.

Let the horizontal diameter be AB, becauſe of the equal rays AC, CB, an equal weight to B, being applied at A, will make an *equilibrium* to the weight B.

Beſides, the weights which hang at the extremities of a balance being between them, as is demonſtrated in the mechanicks, in a reciprocal *ratio* of their diſtance to the point of ſupport, a weight which ſhall be at the weight B, as CD, equal to the right ſine of the angle GCL, to the ray CB, will make an *equilibrium* to the weight G.

By the ſame reaſon a weight which ſhall be to the weight B, as CE to CB, will make an *equilibrium* to the weight H; another weight which ſhall be to B, as CF to CB, will make an *equilibrium* to the weight I, and ſo of the reſt in any quantity whatſoever; if the weight L, being in the plumb line, has no need of any weight in A,

* Plate IV. Fig. 3.

to

to make an *equilibrium* to it. Now as in whatſoever place of the circumference of the wheel the reſiſting force is applied, it is always thought to act by the direction of a tangent to the extremity of a ray equal to AC, it follows, that the reſiſting force, or the ſum of the weights which being applied at A, make the *equilibrium* to the given weights is to theſe ſame weights as the ſum of the right ſines of the angles, by which theſe weights are removed from the plumb of the centre of the wheel, to the total ſine multiplied by the number of the weights. Q. E. D.

A deſcription of the commodious way of making uſe of fire to move machines.

All that is ſaid above being ſuppoſed, if * A, B, C, D, E, F, and 1, 2, 3, 4, 5, 6, &. are two circular and concentric ranges of cells diſpoſed about a horizontal and moveable axis G, and exactly cloſed on all ſides, excepting that each of the cells A, B, C, D, E, F, &. may communicate with each of the cells, 1, 2, 3, 4, 5, 6, &. by means of the tubes, HI, LM, NO, PQ, RS, TV, &. and that the cells 1, 2, 3, 4, 5, 6, & have communication with each other by the valves 7, 8, 9, 10, 11, 12, &. which are all placed and open the ſame way, ſo that they permit the water to enter out of the firſt into the ſecond cell, out of the ſecond into the third, and ſo on to the laſt, and again from this laſt to the firſt; but refuſe it entrance the contrary way.

If moreover the cells A, B, C, D, E, F, &. have no communication directly with one another, and 2 of the cells 1, 2, 3, 4, &. have 2 apertures, as X, ϒ, by which they have been filled with water, and theſe apertures have afterwards

* Plate IV. Fig. 4.

been

been closed exactly, if the capacity of each of these cells, 1, 2, 3, 4, 5, 6, &c. is near $\frac{1}{7}$ of that of the cells A, B, C, D, E, F, &c. and if the resisting force is supposed to make an *equilibrium* with the water of the cells, 1, 2; if lastly we apply at BB, the flame coming out of the furnace AA, driven by the air which enters by the grate of the furnace, and if the bottom of the barrel made of the cells ABCDEF, &c. dips in the cold water of the reservoir, &c. &c. I say:

1. That the air of the cell B, will increase its spring sufficiently to sustain not only the height of water IΥ, but also to make the water pass out of the cell 1 into the cell 3, if this height IΥ is but near 5 feet.

2. That the weight of this water will make all this construction turn the way BAF, about the centre G, if the resisting force is less than this weight.

3. That as fast as by the motion about the centre G, the water in Υ, aims to descend, new cells present themselves at BB, and new air augments its spring to repel it, and sustain it at the sufficient height, that its weight may be continually superior to the resisting force; during which the air that had been dilated resumes its former bulk, as fast as the cells which contain it, pass thro' the water of the back, &c. &c. and from thence thro' the air, where it ceases to reduce itself, to be anew dilated every time these cells return to BB; for by the first and fourth experiment, the air increases its spring by a quantity equivalent to a load of 11 feet 8 inches in height of water: Now of these 11 feet 8 inches, according to M. *Mariotte*'s rule for the pressure of the air, there are but 10 inches and $\frac{1}{2}$ employed in the pressure

necessary

neceſſary to make place for the augmentation of the bulks of air, dilated by the heat of the fire, which added to 5 feet 6 inches $\frac{1}{7}$, which this augmentation makes the ſpring of the dilated air to loſe, by Exp. I. Cor. 5. make in all 6 feet, 4 inches, 10 lines; which being taken from 11 feet, 8 inches, there remain 5 feet, 3 inches, 2 lines in height, to which the ſpring of the air dilated by a heat equal to that of boiling water can ſuſtain the water in Y.

Now by the preceding problem, the reſiſting force being applied in any place of the circumference, which paſſes thro' the middle of the cells 1, 2, 3, 4, 5, 6, &. is to the weight of this water, almoſt as 11 to 14; and if this weight is 12000, this reſiſting force will be $9428\frac{4}{7}$. Now in allowing 12 feet of diameter to the barrel made of the cells 1, 2, 3, 4, 5, 6, &. to a like length of 12 feet, and 2 feet of depth taken on the ſide of the centre of the wheel, theſe cells contain a ſpace of 754 cubical feet $\frac{2}{7}$ of which $\frac{1}{4}$ is $188\frac{4}{7}$, which being multiplied by 70 *lb.* weight of a cubical foot of water, make 13200 *lb.* But as in machines the moving force ought to be ſuperior to the reſiſting force, and as there is always ſome friction to ſurmount, the 1200 *lb.* muſt be counted for that; ſo that the effect of this motion may be reputed $9428\frac{4}{7}$, which would be equivalent at leaſt by *Exp.* V. to the force of 157 horſes, if the reſiſting force made a league of way in an hour; but as to do this, the conſtruction had need to make 400 revolutions in an hour, that is, employ but 9 ſeconds in each, whereas by *Exp.* III. it cannot employ leſs than 36, it follows that this mean would at leaſt be inſtead of 39 horſes, or of 234 men, by *Exp.* VI; which ſuppoſes the degree of heat, equal to that

of boiling water, for otherwiſe the effect would become ſo much more conſiderable, as this degree of heat would be greater; and the profit in making uſe of this ſort of fire-mill, would be ſo much the more conſiderable, as the price of wood would be leſs than that of labour; and beſides the furnaces might be alſo applied to other purpoſes, as vitrifications, founding of metals, and other chymical operations, or mechanical works, that demand the aſſiſtance of fire.

I ſhall not enter into a farther detail of the conſtruction of this machine, only I ſhall juſt mention, that it would be proper to make the cells which contain the air of great plates of copper rivetted and luted; and that all the reſt, except the tubes of communication, may be of wood.

The advantages of this way are:

1. That we can ſtop and reſume the labour at pleaſure, without being encumber'd with the care and feeding of the horſes, and without being ſubjected to the danger of loſing them.

2. That we have always an equal and uninterrupted power, when we pleaſe, which cannot be had in wind-mills and water-mills, the firſt being often ſtopped for want of wind, and the others by the ice and inundations of the water.

3. That we are not confined to any places, becauſe combuſtible materials are found almoſt every where.

The deſcription of Fig. 4.

The fire at BB dilates the air incloſed in A, and making it paſs thro' the canal HI, preſſing the ſurface of the water in *i*, makes the valve or ſucker 18 cloſe itſelf, and the valves, 7, 8, and 9 open to let the water riſe towards Y, and charge

this ſide, which makes the wheel move on its centre G, and the cell B ſucceed the cell A, whilſt this laſt enters the water, &c. to make the air which it contains return to its firſt ſtate.

And it has been demonſtrated, that in making Q 19 of 8 feet, 20, 21 of 12, 22 L of 18 and 23, 24 of 30, the whole on a depth of 12 feet, and that the heat at BB is equal to that of boiling water; it will anſwer to 39 horſes.

V. *A deſcription of a level which is uſed by M.* Couplet, *more exact in this ſecond edition.**

This level † is compoſed of a teleſcope AB, of 2 veſſels CD, joined together by one or two tubes *o o*, which I call the canal; of 2 other veſſels *o b*, *o c*, called *callebaſſes*, becauſe they carry the teleſcope, and float in the water with which this canal is filled. The teleſcope is 2 feet 8 inches long; its tube ſhould be of tin, tho' the reſt is made of copper: in one end A of this tube, as in all the other tubes of teleſcopes, the object glaſs is placed; and in the other end 3 eye glaſſes are placed, if we would have the teleſcope conſiſt of 4 lens's. One eye-glaſs alone would be ſufficient here, becauſe it does not ſignify whether the object is inverted or not. The opticians uſually put the 3 eye-glaſſes into little ſcrew-boxes, which they glue and fit in a tube of paſtboard called *porte oculaire*, and they puſh it into the end B of the teleſcope.

However the teleſcope of the level differs from the others only in having a bar on the inſide, which traverſes it diametrically and horizontally. ‖ This bar is nothing but a hair ſtretched in ſuch a place *c*, that he who looks thro' the teleſcope

* *June* 27. 1699. † Plate V. Fig. 1. ‖ Fig. 2.

ſees it very plainly on the objects to which he points it.

This hair is ſtretched in the middle of a ring or crown of tin 4, the greateſt diameter of which is equal to that of the eye-glaſſes of the teleſcope, that it may be put into a ſcrew-box as each eye-glaſs. On the edge of this crown are made two little inciſions, which are covered with ſome drops of wax, after having twiſted the ends of the hair.

But becauſe we muſt ſometimes turn and return the *porte-oculaire* to place the horizontal hair, and this turning would divide or unglue the boxes 5, I put this *porte-oculaire* into one end of a tin tube 6, which I call *foureau du porte-oculaire*; and at the end of this *foureau*, I ſolder a ſort of funnel which may be ſqueezed to turn the *porte-oculaire* juſt as one will; and I put this *foureau* with the *porte-oculaire* in the end B of the teleſcope.

It is eaſy to conclude, that the end B of the teleſcope is much heavier than the end A; to counterbalance this greater weight, a leaden ring is ſoldered at the end A.

At * CD is a vertical ſection of the two veſſels † CD. Theſe veſſels, a little above the middle of each of their ſquare faces, have points entering within, about a line, and a little ſoftened; the water, with which theſe veſſels are filled, has a communication with both of them by two tubes ‡ *o o*, the ends of which are ſoldered above the joining of their ſquare parts with their truncated pyramidal parts: one tube might ſuffice, but two maintain theſe veſſels better.

The calebaſſes ‖ *ob*, *oc*, in their cylindrical part, are leſs by three or four lines than each ſide of the ſquares CD, that they may float at liberty between the points of each of theſe veſſels. Their

* Fig. 3. † Fig. 2. ‡ Fig. 3. ‖ Fig. 1.

height,

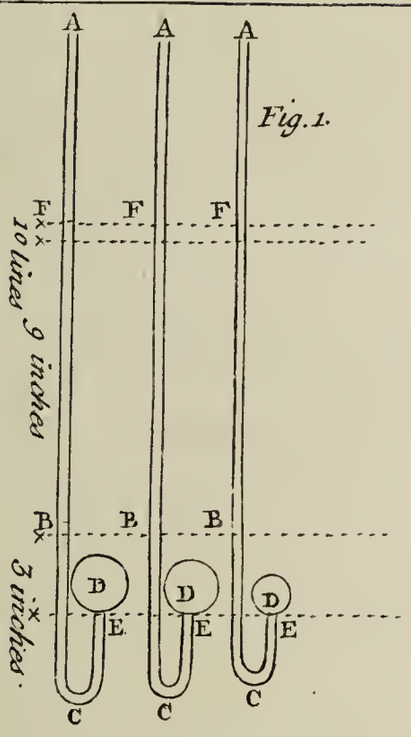


Fig. 1.

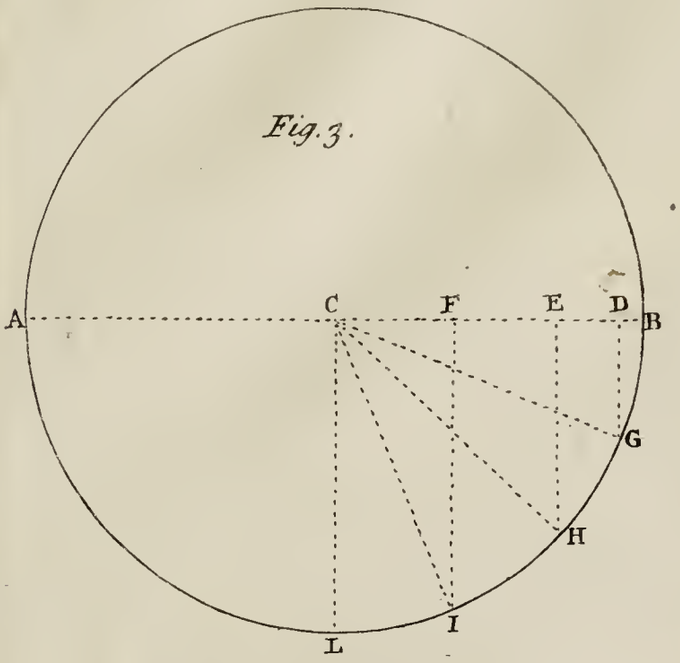


Fig. 3.

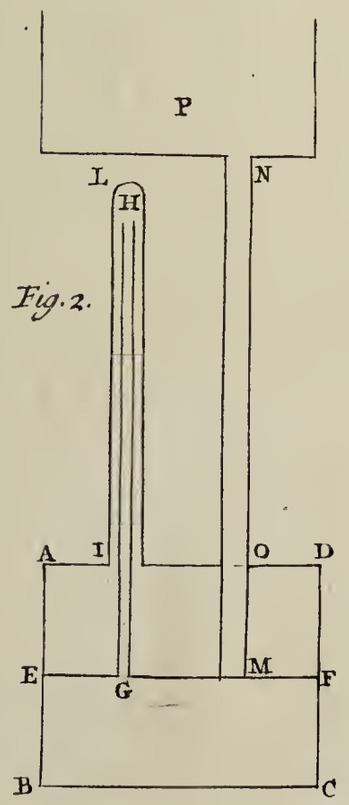
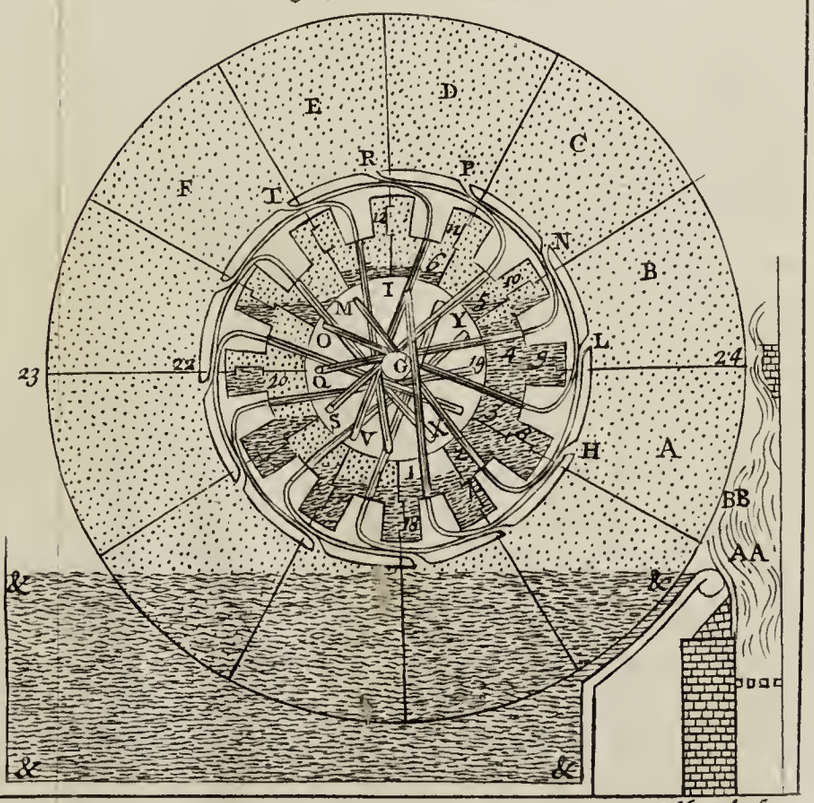


Fig. 2.

Fig. 4. The Fire Mill



J. Mynde sc.

height, to the edge of their lid, (which muſt be a very flat dome, without being enough ſo to ſee the water reſt upon it) is leſs by about two inches than the depth of the veſſels CD: theſe calebaſſes muſt be ſo well ſoldered, that not a drop of water can get into them; and to be ſure of this abſolutely neceſſary condition, they muſt be tinned on the inſide before they are covered with their domes.

When they are covered, the canal is filled with water; they are put in; and to make them float even, ſome lead muſt be drop'd thro' a little hole in their domes, and then theſe holes muſt be cloſed with wax. I have choſen this ſort of ballaſt, becauſe notwithſtanding any bruiſe, or other accident that might happen to theſe calebaſſes, they might be made to float always even by ſhaking them, to make the too great quantity of lead on one ſide roll to the other, which could not be done, if they were charged with any liquor. If they were found from any ſlight cauſes to leave floating even, they might be immerged deeper by dropping in more lead.

The tube of the teleſcope AB, which is placed on the domes of the calebaſſes, muſt be, as we have ſaid, of tin, which is much lighter than copper, to have their greateſt weight below the centre of gravity of theſe calebaſſes; and thereby to render them more capable of reſiſting what would hinder them from floating even.

To ſeat the tube of the teleſcope on the domes *ob*, *oc* of the calebaſſes, you muſt endeavour to hold it there for a moment, whilſt you mark the place where the hooks are to be ſoldered, as may be ſeen under A, B, and better in * 8. Theſe hooks have an opening of about a line, which is

* Fig. 7.

pro-

prolonged to ſome lines near the domes; and it is near the ſoldering of theſe hooks upon the domes that the holes are, by which theſe calebaſſes are filled.

The hooks being ſoldered, the teleſcope being put in, its end B being ſet as far diſtant from the end of the canal as is neceſſary to put the eye to the eye-glaſs of this teleſcope, and having obſerved that this poſition of the teleſcope does not hinder the calebaſſes from floating even and very freely among the four entering points, there are made thro' the ſlits of theſe hooks four *repairs* on the tube of the teleſcope; on theſe *repairs* are ſoldered wings * 7, filed and adjuſted in ſuch a manner, that they may enter freely to the very bottom of the ſlits of the hooks, and bear the teleſcope equally to ſome lines above the bottom of theſe hooks. Theſe four wings are formed with a ſharp bottom, and the bottom of the four ſlits is ſmoothed with a fine-toothed file: theſe wings ſerve to hold the teleſcope in the ſame poſition on the calebaſſes.

M † is the ſection of a portion of a ſphere taken in a trunk of aſh, or other proper wood. Under this portion of a ſphere, or in the ſection repreſented by *o*, *p*, *q*, the diameter of which ſhould not exceed the height of the canal, three branches of wrought iron are faſtened together, having at each of their ends a hinge and a ſcrew; theſe ſcrews *p*, *o*, *q*, enter into three ſockets at the ends of the three ſupporters of the ſphere M. As for the other ends, there is no neceſſity that they ſhould be ſcrewed.

This pedeſtal being placed almoſt horizontally, which is done by moving one of the legs nearer or farther off, the ſupport ‖ ESF of the canal is

* Fig. 3. † Fig. 6. ‖ Fig. 4.

to

to be placed upon them. It was at firſt a plank about two inches thick, equal in breadth to the diameter of the portion of the ſphere M; its plan ESF, and its profile * G*r* K*t*H ſhew how this plank has been finiſhed by hollowing the ſquares † E, F, or ‖ G, H, for the veſſels ** C, D to go exactly in, up to *o*, *o*. The ſides of theſe ſquares, which are at the two ends of the ſupport, are only locked with an iron ring, put about the ends of the ſupport, not ſo much to ſtrengthen theſe ends, as to keep the ſupport from being longer than the canal which it bears. This profile ſhews alſo, that this ſupport is rendered lighter, by making in its thickneſs a cavity *r*, K, *t*, ſo deep, that only the edge *r*, *t*, of its circumference rubs againſt the convexity of the portion of a ſphere †† M, that it may not have any more frictions to overcome, when the inſtrument ſhall be turued to either ſide upon this ſupport; there are other parts of this ſupport ſcooped away alſo from ‡ G to *r*, and from *t*, to H, to give it the better play.

Before the three branches of iron ‖* *o*, *p*, *q*, are faſtened under the portion of a ſphere M, it is traverſed by a great iron pin long enough to go thro' the thickneſs of the ſupport, and the hole *† K or ||| S is made larger than is neceſſary for the thickneſs of this pin, that the ſupport may be drawn to either ſide on its pedeſtal *‡ M, to hinder the water, with which the canal is filled, from being ſpilt over either of its ſides.

The inſtrument is then prepared in a convenient place to diſcover ſeveral very diſtant objects,

* Fig. 5. † Fig. 4. ‖ Fig. 5. ** Fig. 1.
†† Fig. 6. ‡ Fig. 5. ‖* Fig. 6. *† Fig. 5. ||| Fig 4.
*‡ Fig. 6.

but

but not ſo far off that we cannot well diſtinguiſh whether the hair in the teleſcope reſts upon the top * 5 of ſome building, upon the funnel 7 of ſome chimney, upon ſome crowning of a work 8; or laſtly, upon ſome elevation well limited and diſtinguiſhed; for whereſoever the hair reſts, it has always little vibrations, which leave a little thread of light between it and the greateſt height of the objects to which it is pointed, whereby we may judge the better whether the hair exactly reſt upon them. Theſe objects thus choſen are ſo much the more neceſſary, as a hair which is but $\frac{1}{24}$ of a line in thickneſs covers about a foot of the object, which is near 3000 toiſes from the obſerver.

To ſee whether the hair conſtantly touches the ſame object, its reſt is interrupted by gently ſhaking the canal, and obſerving whether it reſumes the ſame place; or for a ſtronger proof, the teleſcope is taken out of its hooks and put in again.

Care muſt be taken that there be no water on the domes of the calebaſſes, or any thing that hinders the wings † 7 from reſting on the bottoms of the hooks; that the water in the canal is high enough, that there may be no danger of the calebaſſes touching at the bottom, or either end of the teleſcope on either edge of the canal.

If one ſhould ſee, that among ſeveral obſervations there was one where the hair did not return exactly on the ſame object, one might ſuſpect that the difference came from the refractions, which differ often and from one moment to another; eſpecially when it rains, when it is very hot or very cold, when the viſual ray, which parts from the inſtrument, paſſes from a place where there is a fog, to one where there is not.

* Fig. 8. † Fig. 3.

Tho'

Tho' the hair ſhould return conſtantly to the ſame object, yet we cannot conclude, that this object is in a level with the eye of the obſerver; but only that the viſual rays * H 9 directed by this inſtrument always make equal angles 9HO, with a plumb line HO, which is imagined to deſcend from the eye of the obſerver to the centre of the earth.

With this inſtrument put in the ſtate juſt mentioned, we may find as many true points of level between them as we deſire, provided the places to which the teleſcope is pointed, are equally diſtant from the inſtrument.

To have only two, which we ſhall mention how all the perfection may be given that can be deſired, plant upright a ſtaff *c*4, at 300 or 400 toiſes from the inſtrument, on the bank of a river, a lake, or a canal, along which your aſſiſtant ſhall preſent you a ſignal, or a little ſquare of iron, white on one ſide, and black on the other; for in any ſituation whatſoever the black is ſeen more eaſily than the white: let him raiſe or lower this ſignal, till the hair of your teleſcope juſt touches its upper edge 4, and reckon how many feet, inches, and lines there are from the edge 4 to the baſe of the ſtaff *c*; then let the aſſiſtant preſent in like manner in another place *e*, as far diſtant from the inſtrument as *c*, another ſignal 5; and knowing by your ſign that the hair of your teleſcope touches the edge of his ſignal, he writes how many feet, inches, and lines there are from this edge 5 to the baſe *e* of his ſtaff.

By means of theſe points 4 and 5, which it is eaſy to find by all ſorts of levels, how defective ſoever they may be, the inſtrument may be adjuſted in the following manner, and diſengaged

* Fig 8.

from the troubleſome neceſſity of always chooſing points equally diſtant from it.

Cauſe the inſtrument to be carried near one of the ſtaves *c* 4, or *e* 5, and count how many feet, inches, and lines there are from the upper edge of the ſignal 4 to *p*, where you ſhall have obſerved the centre of the object-glaſs of your teleſcope to be, or the hair to be cut by the ſtaff *c* 4: let your aſſiſtant then go and place upon the ſtaff *e* 5 a ſignal *q*, as much below 5 as *p* is below 4: then turn the object-glaſs of the teleſcope of your inſtrument towards *q*, without making any alteration in the pedeſtal of this inſtrument.

We ſee that the proper places to plant theſe two ſtaves, ſhould be choſen along a river, that their baſes may not be far from being upon a level. Having thus on each of theſe ſtaves two other points *p*, *q*, on a true level, as the points 4 and 5 are, obſerve whether the hair of the teleſcope, which is at *p*, touches the upper edge of the ſignal *q*. If it touches it, the inſtrument is perfect; but it is a great chance. If the hair reſts higher than *q*, take ſome of the lead out of the calebaſs *oc*; or if it did not ſeem too much immerged in the water of the canal, put ſome in the calebaſs *o b*, which bears the object-glaſs. In ſhort, take out of one, or put into the other, till the hair of the teleſcope approaches the ſignal *q*; then begin again to obſerve where the hair of the teleſcope is cut by the ſtaff *c* 4; reckon, as in the preceding operation, how many feet, inches, and lines there are from the ſignal 4 to this point, and place on the ſtaff *e* 5 a ſignal as much below 5, as the centre of the teleſcope is found to be below 4; and if you obſerve the hair to touch the edge of this ſignal, you may conclude this inſtrument to be perfect; if not, leſſen by degrees

with

with a knife, and not with a file, the leaden ring ſoldered at * A ; charge or diſcharge the calebaſſes, and repeat theſe operations, till the meaſure of the point of the level † 4, in the centre of the teleſcope, is equal to the meaſure of the point of level 5, where its hair cuts the ſtaff *e* 5 ; and then you will be ſure, that the viſual rays which the inſtrument directs, whether long or ſhort, make right angles with lines HO, imagined to deſcend from the eye of the obſerver to the centre of the earth. This is all that can be required in the moſt juſt inſtrument that ever was ſeen.

The ſcale ‖ gives the meaſures of each part of this level. If the length of the ſupport, and that of its three feet are reduced to the length of the canal, and if the diameter of the portion of a ſphere is reduced to the breadth of the ſupport, and to the height of the two veſſels of the canal, it is only to incloſe the whole in one caſe.

VI. *A method to centre teleſcope-glaſſes in working them, by M.* de la Hire ‡, *at the obſervatory.*

After what I have ſaid on the manner of knowing the inequality of the thickneſs of the glaſſes, uſed to make the object-glaſſes of teleſcopes, it will not be difficult to centre them in working ; that is, to cauſe the greateſt thickneſs of the glaſs to be found in the centre of the figure, when it ſhall be wrought.

Firſt, the piece of glaſs, of which an object-glaſs is to be made, being cut in a circular figure,

* Fig. 1 and 2. † Fig. 8. ‖ Fig. 9.
‡ July 22, 1699.

mark the centre as at * C: and by the method which I have given, and as I ſhall afterwards explain, trace upon this circle the diameter AB, which will determine its greateſt thickneſs in B, and its leaſt in A.

Secondly, begin to form the glaſs according to the figure which you would give it, by gradually diminiſhing the part B, ſo much as you can gueſs it to be thicker than the part A; and this ſide of the glaſs being at laſt entirely finiſhed and poliſhed, unglue it, and ſearch again for the thickeſt part, if it is not equal every where. But as it is cut on one ſide, the centre may be determined by the ſame method; that is, mark the point where the greateſt thickneſs of this glaſs is. This will be done by tracing a diameter firſt, as I have juſt ſhewed, in which a clear or black line does not appear multiplied, which may always be found; and if this line does not appear doubled in all the diameters, we are ſure that the glaſs is well centered, and that we may work it equally on the other ſide to give it its entire perfection. But if we trace on the glaſs another diameter DE, perpendicular to AB, which is that where the image of the clear or black line does not appear multiplied, and the image of the ſame line does appear multiplied in the diameter DE, we ſhall know that the glaſs is not centered. We muſt then make the image of the line on the glaſs, which is parallel to the diameter DE, appear, and make it move thereon till it no longer appears doubled, which we may always find, and this line muſt be marked as at FG on the ſurface of the glaſs, which will cut at right angles the diameter AB at the point H, which will be then the centre of this glaſs. Now tranſport the magnitude CH to CI on the other ſide

* Plate VI. Fig. 1.

of

Fig. 1.

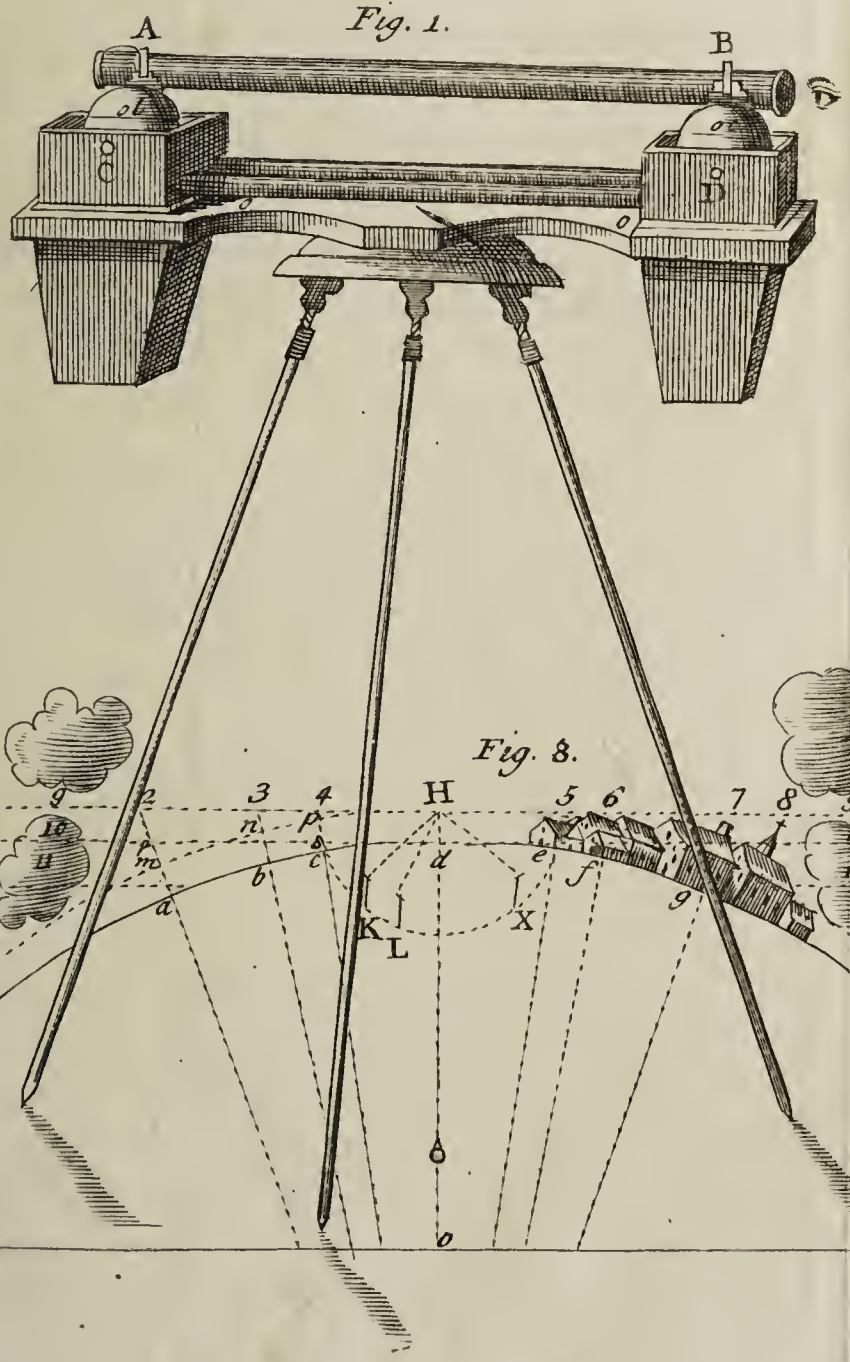


Fig. 2.

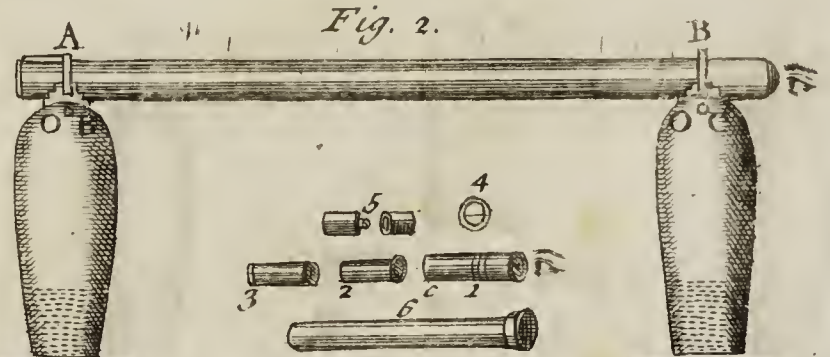


Fig. 3.

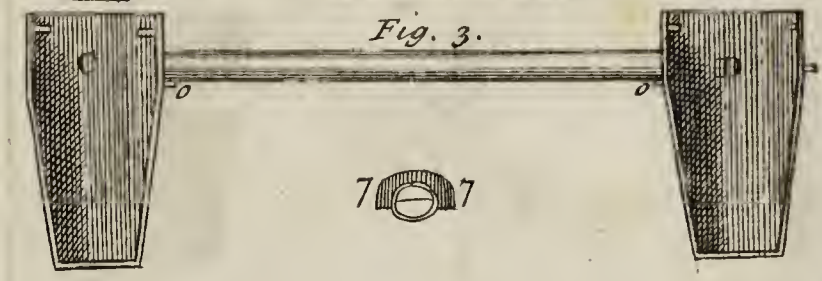


Fig. 4.

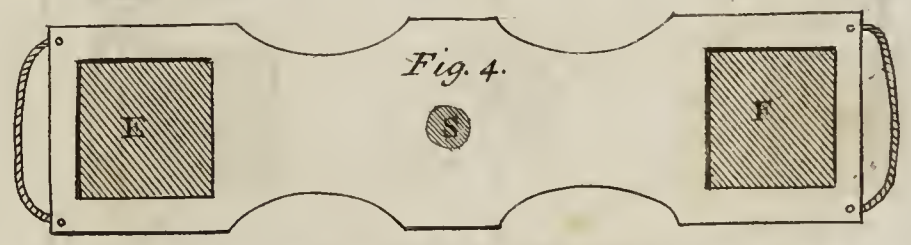


Fig. 5.

Fig. 8.

Fig. 7.

Fig. 6.

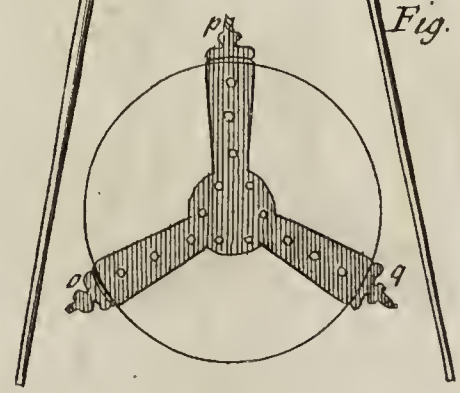
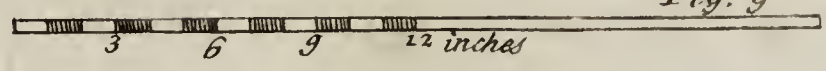


Fig. 9



of the centre of the diameter AB, and having faſtened the glaſs with maſtick as before, begin to cut it on the ſide which is ſtill flat, and wear away gradually its part towards B, more than that towards A; ſo that being almoſt cut all ſpherically, there remains only the little point I, which is not cut on the ſurface. Then this ſide of the glaſs may be finiſhed, and you may be ſure that it will be well centered.

On the ſurface of the glaſs, which is firſt cut, a little bit of white paper may be paſted, where the point I is marked, that this point may always be known when you cut the other ſide.

The demonſtration of this practice is not difficult: for let *ABMN be the ſection of the glaſs by the diameter AB of its figure, and perpendicularly to the plain ſurface AB, if we imagine a plane parallel to this ſurface, which touches it in O the ſide NOM of the glaſs which is wrought, it is evident that the point O will be the centre of the glaſs, or its oppoſite point H; and ſeeing the point C is in the middle of the diameter AB, if we take CI equal to CH, and make the point I be the place where the curve PIQ equal and ſimilar to NOM, touches the plane AB, the line IO being biſected at K, will give the point K, for the centre of this glaſs. Q. E. D.

Now as for the manner of finding the greateſt thickneſs of the glaſs, as it depends only on one multiplication, you muſt only make uſe of the reflection of a bright line in the dark, or of a black one in a ſtrong light. We muſt therefore expoſe one ſide of the glaſs † AB to the black or bright line RS, ſo that it may be perpendicular to the plane, which paſſes through the eye O, and the line RS, and that this ſurface of the glaſs

* Fig. 2. † Fig. 3.

may

may be alſo very little inclined to this line to be able to perceive the image of it more eaſily; and when the image AB of the line RS appears ſingle, you will know that the diameter AB of the circular piece of glaſs will thereby determine its greateſt and leaſt thickneſs. But if the image AB appears double, and the moſt vivid is towards X, and the fainteſt towards Y, then the part X will be thicker than the part Y, as I have already demonſtrated.

VII. *Obſervations on the inſects called adder-bolts or dragon-flies, by* M. Homberg.*

As my obſervations were made only on one ſpecies of adder-bolts, I ſhall endeavour to diſtinguiſh it from the reſt in the firſt place, and deſcribe the parts which principally belong to my obſervation.

The males and females are of the ſame bigneſs, being about 20 lines long: the body of each of them is equally ſlender; except that the end of the tail, or extremity of the belly in the female † *b* is a little bigger than that of the male ‡ *a*. They are both very lively, and keep uſually on the banks of rivers.

The ‖ males are of a ſhining violet colour all over their bodies; their 4 wings are tranſparent, a little gilded, with a great ſpot almoſt in the middle of each wing *e*, of the ſame violet colour with their whole body, which makes this part of the wings opake.

The ‖‖ females are all over their body of a ſhining gilded grey, approaching to green. Their 4 wings *f* are tranſparent, of the ſame colour, and without any ſpot.

* *Aug.* 22, 1699. † Plate VI. Fig. 5.
‡ Fig. 4. ‖ Fig. 4. ‖‖ Fig. 5.

When they are at reſt, or do not fly, their 4 wings draw cloſe to each other, ſo as to ſeem but one, whereas ſeveral other ſpecies of adder-bolts keep their wings always extended, as well when they are at reſt as when they fly.

The head of this animal, which is very big in compariſon of its body, joins to the breaſt only by a very ſlender thread: its belly * *a c*, or that part which reaches from the place where its wings are planted, to the other extremity, is divided into 10 joints, the motion of which is only up and down, and from ſide to ſide.

The place, on which the wings are planted, I ſhall call the breaſt.

It has its lungs about the middle of the belly towards † *b*, which appears by this part's heaving continually at ſmall intervals, as the parts of reſpiration commonly do.

The extremity of the belly of the male ‖ *a*, or the tenth joint of its belly, is a ſimple ring, which makes its *anus*; it is furniſhed with 4 little hooks, 2 large above, about a line in length, and 2 ſmaller below, which it can open and ſhut, juſt as lobſters do their claws.

The extremity of the belly of the female ‡ *b*, ſeems to conſiſt of 2 tubes placed one below the other. The upper one is the *anus*, which is placed as in the males, and the lower one is the female organ, or entrance into the *matrix*. This laſt is about a line long; and riſes from the lower part of the eighth joint of the belly: each of theſe tubes are furniſhed at the end with 2 very ſmall points; whereas the *anus* of the male has 4 hooks. The 2 ends of the tube, being placed one above the other, make the extremity of the female's belly bigger, and not to be ſo much pointed as in the male. I

* Fig 4. † Fig. 4. ‖ Fig. 5. ‡ Fig. 5.

I have ſeen theſe animals perform an action, which has apppeared to me very extraordinary, and has raiſed a curioſity in me to examine them attentively; the male finding the female ſitting on ſome leaf or branch by the water-ſide, took her as he flew, with the hooks of his *anus*, by the neck between the head and breaſt, and ſo carried her off hanging by the head at the end of his tail.

I thought at firſt, they had been two different ſpecies of animals, and that they were quarrelling; but as I ſaw no reſiſtance made by one of them againſt its being carried off, but on the contrary, that one of them preſented itſelf, and ſeemed to expect the other, to be carried off more commodiouſly, I thought quite otherwiſe.

I followed them, and ſaw that the male ſeated himſelf not far off, on a leaf of a ruſh, and at the ſame time he raiſed his tail, with which he held the female by the neck, to place her on the ſame leaf where he was. The female being thus ſeated behind the male, bent her belly, turning it between her legs, and with the end of her belly, ſhe thruſt her parts againſt the breaſt of the male, whoſe genital parts are in that place *: the male ſupporting the head of the female with his tail, during this whole action.

They continued in this poſture for about three minutes, and then the male lifted up his breaſt ſtrongly, and the genital parts of theſe two animals ſeparated, as if they had been forced aſunder: the male alſo let the head of the female go at the ſame time, and flew away immediately.

The female being at liberty, put herſelf in order again, and remained without motion in the ſame place for a full half quarter of an hour, and then ſhe flew away alſo.

* Fig. 6.

I have caught ſeveral of theſe animals, to examine their genital parts; and this is what I have found. The upper part of the belly, in both ſexes, is convex thro' its whole length. The under part of the belly is folded and bent inwards, and forms a gutter lengthwiſe, almoſt like the inner part of a feather between its vanes. This gutter begins in the males at the third joint, and is continued quite to the *anus*. The firſt joint of its belly, next the breaſt, is only a round and very narrow ring, about the breadth of a large pin; and it does not ſeem to have any other uſe, than to give a more free and large motion to the reſt of the belly.

The ſecond joint in the males * *c*, is two lines long, very much hollowed in the fore part underneath, making a ſort of a bag, the edges of which are ſet with hair, and the bottom is towards the breaſt.

From the bottom of this bag proceeds a ſmall hard and black body, of the ſize of a hog's briſtle, two lines long, with a hard and very ſmall white pearl at the end. This little body ſeems to be implanted into the breaſt of the male, and to perform the office of a *penis*. It is lodged lengthwiſe in the bag, in ſuch a manner, that the little white pearl is always viſible; when we preſs the end of a feather into this bag, the *penis* comes out of itſelf, about a line in length; which happens alſo when the *anus* is preſſed. I have cut the breaſt of the male tranſverſly with ſciſſars above the wings, and found in the fleſhy part within the breaſt a hollow cone, the baſe of which was towards the head of the animal, and its *vortex* bordered within on the root of the *penis*; I have driven a little needle into the *vertex* of this hol-

* Fig. 4.

low cone, which has made the whole length of the *penis* come out of the bag.

I have opened the breaſt in ſeveral males to examine this cavity, but I have found it only in two; all the reſt had their breaſts full. One of theſe had juſt done coupling when I took him, and the other I took at hazard. This difference made me think, that this cavity might be the reſervoir of the ſeed of this animal, which being but juſt emptied, made the cavity ſtill viſible; but before the copulation, this place being full, or ſometime after it, the ſides of this veſſel being fallen, there would not appear any ſenſible mark of it.

The bag, in which the *penis* is lodged, is only a continuation of the gutter which runs along the whole belly almoſt underneath, with the difference, that in this place the gutter is deeper and wider, than in all the reſt of its extent, and that it is hairy, whereas all the reſt is ſmooth.

The under part of the belly of the females is in like manner folded into a gutter, which begins at the ſecond joint of the belly * *d*, which is not hairy, like the males, and continues along the 6 following joints:

The two laſt joints but one of the female hold its external genital parts *b* underneath. They are formed after the following manner: the ninth joint underneath has an aperture furniſhed on each ſide with a light grey little wing. Theſe two little wings cover this aperture, and have a motion of opening and ſhutting; and when they are ſhut, they ſeem to form a little tube *i*.

At the root of the eighth joint, there ariſes a little bunch, up to the root of the ninth joint. On the extremity of this bunch are planted two little crooked horns, black, very hard, a little above a line long, ſhaped almoſt like the fangs of

* Fig. 5. a

a viper, but a little more crooked, and its points are turned towards the *anus*. They are articulated, and have a motion every way; they are uſually lodged under the little wings juſt deſcribed, and are entirely hidden by them; they are laid ſo near each other, that they appear like one ſingle hook.

I believe theſe two little horns may have the two following uſes. Firſt, as they are couched between the two little wings, which cover the female parts, and as they have a motion every way, they can, by parting from each other, open the two little wings, and thereby diſcover the aperture of theſe parts.

The ſecond uſe may be, to direct the parts of the female in copulation towards thoſe of the male, and that in the following manner:

We have ſeen that the parts of the male are very near its breaſt, whereas thoſe of the female are placed at the other extremity of the belly, ſo that the female is obliged, in copulation, to bend her belly and bring it between her legs and under her breaſt, to reach the parts of the male, as is repreſented in *Fig.* 6. which is a very uneaſy poſture, in which ſhe might often miſs the parts of the male, without the aſſiſtance of theſe two horns: but when theſe horns riſe under the little wings, they preſent their convexity to the gutter, which poſſeſſes the whole under part of the belly of the male, in which they very eaſily engage themſelves; and after they are entered into this gutter, they ſerve for an infallible guide to the parts of the female, to arrive ſurely to thoſe of the male.

I have ſhut up ſeveral of theſe females, to ſee if they would produce any eggs; but as they ſtood in need of nouriſhment, which they would

not take in their priſon, they all died, ſo that I could not extend my obſervation any farther.

I never found any eggs in them on diſſection, which makes me believe the females hide themſelves ſoon after copulation, to lay their eggs, and that they periſh afterwards. The males alſo muſt periſh ſoon after copulation; which I gueſs, becauſe I have found a great many wings of males in ſeveral places, where it is probable, that they died; and as I found no bodies, it is probable, that they were eaten by other inſects.

I obſerved, that the firſt of theſe animals, which I took about the 18th of *July* this year, particularly the males, were longer and ſtronger than thoſe which I took a fortnight afterwards, that there were hardly any three weeks after, and that theſe few were very poor ones; which makes me believe, that theſe animals could not all hatch at the ſame time, and that the firſt flight is better than the laſt.

VIII. *An inquiry into the ſtrength of man to move burdens, either by lifting, carrying, or drawing, conſidered abſolutely, and with compariſon to that of animals which carry and draw, as horſes, by M.* de la Hire, *at the obſervatory* *.

I ſuppoſe, in the firſt place, that a ſtrong man of a midling ſtature weighs 140 *lb.* of our weight.

I conſider firſt, that ſuch a man, having his knees on the ground, can raiſe himſelf, by reſting only on the point of the feet, and the two knees being continually joined together; and as

* Nov. 10, 1699.

this

this effort is made by means of the muſcles of the legs and thighs, it is evident, by the ſuppoſition juſt made of his weight, that the muſcles of the legs and thighs will have ſtrength to raiſe 140 *lb.*

But a man having the hams a little bent, can recover himſelf, tho' loaded with a weight of 150 *lb.* together with the weight of his body which he raiſes at the ſame time; ſo that the force of the muſcles of the legs and thighs can raiſe a weight of 290 *lb.* that is, 150 *lb.* of the weight with which it is loaded, and 140 *lb.* of the weight of his body, when the elevation is but two or three inches.

Such a man, as we have already ſuppoſed and conſidered, can alſo raiſe from the earth a weight of 100 *lb.* which ſhall be placed between his legs, by only bending the body, and taking this weight with the hands as with two hooks, and then recovering himſelf. Whence it follows, that the muſcles of the loins alone have ſtrength to raiſe a weight of 170 *lb.* that is, 100 *lb.* of the weight, and 70 *lb.* which is half his own weight; for he muſt not only raiſe the weight of 100 *lb.* but all the upper part of his body, which I eſtimate at 70 *l.* ſeeing he bent down to lift the weight.

As for the force of the arms in drawing, or raiſing a burden, it may be reckoned at 160 *lb.* which depends on the force of the muſcles of the ſhoulders and arms. For if a man takes with both his hands any fixt body, placed above his head, he can eaſily enough, by the ſole effort of his arms, raiſe his whole body, and 20 *lb.* beſides, as if he was loaded with the weight of 20 *lb.* The experiment of this may eaſily be made, if there is a weight of 160 *lb.* which is faſtened to the extremity of a cord, which paſſes over a pully;

and if a man who weighs but 140 *lb.* draws the other extremity of this cord, it is evident that he can never raiſe the weight of 160 *lb.* ſeeing all that he can do is to hang upon this cord, and the weight at the other extremity, weighing more than he, will keep him ſuſpended; for the pully is nothing but a continued ballance with equal arms: but if this man is loaded with a weight of 20 *lb.* he will then make an *equilibrium* with the weight on the other ſide; and if we add to this 20 *lb.* then he will raiſe the weight, for the muſcles of his ſhoulders and arms have ſtrength enough to raiſe all this weight.

Altho' the muſcles of each part of the body, are able to make ſuch great efforts to raiſe burdens, yet we muſt not compute the ſtrength of a man by that of all his muſcles together, tho' the ſpirits which ſwell the muſcles ſerving to motion in general, by contracting themſelves, and drawing the tendons of their extremities, could diſtribute themſelves equally into all theſe parts, and in the ſame manner with a ſeparated part, ſeeing each part uſually ſerves for a ſupport to that which is joined to it. For example, the muſcles of the arms and ſhoulders contracting themſelves, can raiſe a weight of 160 *lb.* But if the body is bent, the arms cannot ſuſtain this weight, unleſs the muſcles of the loins have ſtrength at the ſame time, to ſuſtain the upper part of the body with the weight with which it is loaded; and if the hams were alſo bent, then the muſcles of the legs and thighs, muſt ſtill make a greater effort, as they muſt ſuſtain the weight of 160 *lb.* and at the ſame time that of the whole body. Whence it happens, that in this diſpoſition of the whole body, the ſtrength is divided by the diſtribution of the ſpirits into all the parts, which is

the

the cauſe that a man cannot raiſe from the ground a weight of 160 *lb.*

Not but that there may be ſome men, whoſe ſpirits flow in ſuch abundance, and with ſo much rapidity into their muſcles, that they make efforts triple and quadruple of what is uſual; and this ſeems to me the natural reaſon of the ſurpriſing force that we ſee in ſome men who carry and lift ſuch burdens, as 2 or 3 men together would be at a loſs to ſupport; tho' theſe men are ſometimes of a moderate ſize, and ſeem outwardly rather weak than ſtrong. There was one not long ago, in this country, who was ſaid to carry a great ſmith's anvil, and of whom ſeveral wonderful actions were related: but I ſaw another at *Venice*, who was young, and did not ſeem to be able to carry above 40 or 50 *lb.* with all poſſible advantages, who getting upon a little table raiſed an aſs from the ground, and held it up in the air, by a large girt that went under the belly, and was faſtened at each end to hooks, which hung at the end of 2 little twiſts made of ſmall cords, and a few hairs on each ſide of the head of this young fellow: and all this great ſtrength depended only on the muſcles of the ſhoulders and loins; for he ſtooped at firſt whilſt the hooks were faſtened to the girt; and afterwards he lifted the animal off the ground, leaning his hands upon his knees. He again raiſed in the ſame manner other burdens, which ſeemed heavier than this animal, and ſaid he found leſs difficulty in it, becauſe the aſs ſtruggled when it was off the ground.

I now examine the effort of a man to carry a burden on his ſhoulders; and I ſay the weight of this burden may be 150 *lb.* and that he can walk with this load eaſily enough on a horizontal plane, provided he does not make great ſtrides; but he

cannot

cannot go up a hill or a ladder with the ſame weight. For the action of walking with a burden on the ſhoulders, muſt be conſidered as the circular motion of the centre of gravity of the body, and of the weight joined together, on the foot which advances as centre of the arch of motion; the effort of the muſcles of the other leg ſerving only to puſh this centre forwards; and if the arch deſcribed by this centre is ſmall, the effort of the hinder leg muſt not be great, to make it be deſcribed, ſeeing the whole burden of the body, and of the weight, is not to be raiſed by more than the quantity of the verſed ſine of half the arch; which is not conſiderable in this caſe, with regard to the arch which is the path in which the whole burden advances.

Thus we ſee that a man well loaded can walk the more eaſily, as he makes leſs ſtrides, ſeeing the ſine will be ſo much the leſs, and that he could not advance by making ſuch large ſtrides, that the effort of the hinder leg could not raiſe the burden of the body, and of the weight by the quantity of the verſed ſine of the arch, which would be half the ſtep.

It is eaſy alſo to ſee that the ſame man cannot go up a ladder, or ſteep hill with this load, ſeeing according to what we have already explained, the effort of the muſcles of his legs being able to raiſe a weight of 150 *lb.* only to the height of 2 or 3 inches, he could not raiſe it to 5 inches which is the height of the uſual ſteps, nor go up a hill without making ſuch ſhort ſteps, as to riſe but 2 or 3 inches at each.

It therefore now remains for me only to conſider the effort of a man to draw or puſh horizontally. But to render this explication more clear and intelligible, I conſider his ſtrength applied to the handle

handle of a roller the *axis* of which is horizontal, and over which a cord is turned which ſuſtains a weight, having ſuppoſed the diſtance from the centre of the roller to the elbow of the handle, equal to the ſemi-diameter of the roller, that the force may be compared being applied without any augmentation on the ſide of the machine, I have alſo had no regard to the frictions of the *axis* of the roller, nor to the difficulty which the cord may have to bend.

Firſt it is evident, that if the elbow of the handle is placed horizontally, and that it is about the height of the knees, the effort of the man who raiſes it in drawing, may at the ſame time lift the weight of 150 *lb.* which ſhall be faſtened to the extremity of the cord, taking all poſſible advantages, ſeeing it is the ſame as to raiſe the weight, as I have already explained. But if it is to depreſs the handle, his effort can be no more than 140 *lb.* which is the weight of his whole body, that he can apply by bearing upon it, unleſs he is loaded; for then he could make a much greater effort.

Seeondly, if the elbow of the handle is placed vertically, and is at the height of the ſhoulders, it is certain that a man can make no effort to turn it, by drawing it or puſhing it with his hands, if the 2 feet are againſt each other, and the body ſtrait, which is repreſented by the line * AP, and if the line of the arms repreſented by AM is horizontal, and makes a right angle with AP, ſince in this poſition, neither the force of the whole body or if its parts, nor its weight, can make any effort to puſh or draw, which is known by mechanicks, for I conſider the breadth of the feet only as a ſingle point P. But if the handle is

* Plate VI. Fig. 7.

higher or lower than the height of the ſhoulders, then the line which goes from the ſhoulders to the hands, which is AM, and that which goes from the ſhoulders to the end of the feet, which here is AP, will make an obtuſe or an acute angle, and the man may have ſome force to draw or puſh the handle; and this force depends wholly on the weight of the body, as is eaſy to know and demonſtrate; and this weight or this force muſt be conſidered as reunited in his centre of gravity, which is near the navel within the body, I ſay that regard muſt be had only to the weight of the body to determine the *equilibrium*, for the effort of the muſcles of the legs and thighs ſerves only to preſerve this *equilibrium* in walking.

* Let the handle D be at the height of the ſhoulders A, and let the centre of gravity of the body be at C, the body being very much inclined towards the handle; but let the end of the feet be at P: we muſt conſider 1ſt this point P as the *fulcrum* of a lever or ſtrait rod PCH, which paſſing thro' the centre of gravity C of the whole body, meets the line of the arms MA at the point H; 2. That this point C of the lever being loaded with the weight of all the body 140 *lb.* with its natural direction, its extremity H is ſuſtained with the horizontal direction MAH; whence it will be eaſy to conclude by mechanicks, what effort the weight of the body at C with its natural direction, can make on the handle according to the horizontal direction DH.

For firſt, let PH be of 240 parts and PC of 80; ſince the effort of all the body at the point C is 140 *lb.* it will be but 80 *lb.* at the point H, as if at the point H there was a weight of 80 *lb.* ſuſpended with its natural direction, which muſt be,

* Fig. 8.

in

in the ſuppoſitions that we have made, perpendicular to MA. Wherefore if we draw from the point of ſupport P the line PF perpendicular to MAF, the weight of 80 *lb.* at H with its natural direction, will be to the effort of the ſame according to the horizontal direction MAH on the handle, in the *ratio* of PF to HF, which very much diminiſhes the effort of the 80 *lb.* in a moderate inclination of the body ACB. And if we ſuppoſe for inſtance, that the line PCH makes with MAF the angle PHF of 70 degrees, the line of the body ACB will then be inclined to the horizon, or with MF, an angle of more than 60 degrees, which is at moſt the inclination in which the body can be to be able to walk; and the ſine of 70 degrees which is PF, will be to the ſine of its complement which is HF, very near as 3 to 1; and conſequently the effort of the 80 *lb.* at H according to the natural direction, will be to that which they make according to the horizontal direction, only $\frac{1}{3}$ of 80 *lb.* which is a little leſs than 27 *lb.*

Thoſe who have not made the experiment of the ſtrength of a man, to puſh horizontally with the arms, or to draw a horizontal cord in walking, the body being inclined forwards, whether the cord be faſtened towards the ſhoulders or about the middle of the body, for the effort will be no greater in the ſame inclination of the body, ſince the ſines of inclination and its complement are always in the ſame *ratio*, cannot perſuade themſelves that the whole ſtrength of a man, is reduced to draw only 27 *lb.* with a horizontal direction.

Not but that a man being bent can ſuſtain a much greater weight than 27 *lb.* ſeeing if the line PH made with HF an angle of 45 degrees, it is certain that the weight of the body would ſuſtain

70 *lb.* but as he would be bent according to line as AB, which would be much more inclined to the horizon than 45 degrees, it is certain that far from being able to walk, he would hardly be able to ſuſtain it.

The ſame demonſtration ſerves alſo to ſhew, that a man will have much more ſtrength to draw in walking backwards than forwards. For in this ſituation of the body, the line * PCH which paſſes from the end of the feet P thro' the centre of gravity C, and whereon depends the augmentation of the force, will be always more inclined to the horizon, than the line of the body ACB, quite contrary to what it was in the preceding poſition.

But this manner of drawing could not be put in practice, unleſs it was only to draw a cord, the man continuing always in the ſame place; and one ſhould not fail of putting one's ſelf in this poſture in ſuch a caſe, for nature and experience have taught us to take always all poſſible advantages in common operations.

It is for the ſame reaſon alſo that our mariners, and in general all who row on the ſea, always pull their oars from before backwards, for they have much more force than if they puſhed them forwards, as the gondeliers of *Venice* do, for which I ſee no other reaſon than that of ſeeing before them; which is much more neceſſary for them than great force, becauſe of the frequent turns they are obliged to make in the canals, and to avoid running againſt one another.

It remains for me in the laſt place to compare the ſtrength of men with that of horſes in drawing, which are the ſtrongeſt of all drawing animals; but as it does not entirely depend upon

* Fig. 9.

their

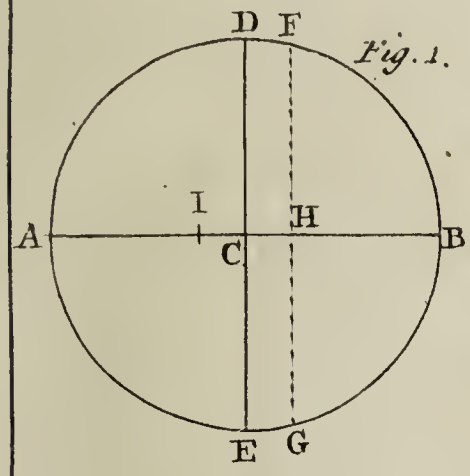


Fig. 1.

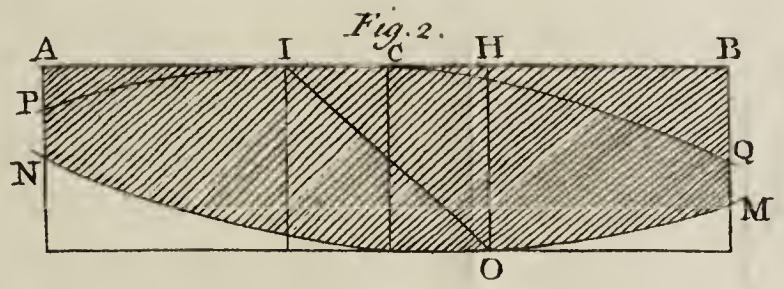


Fig. 2.

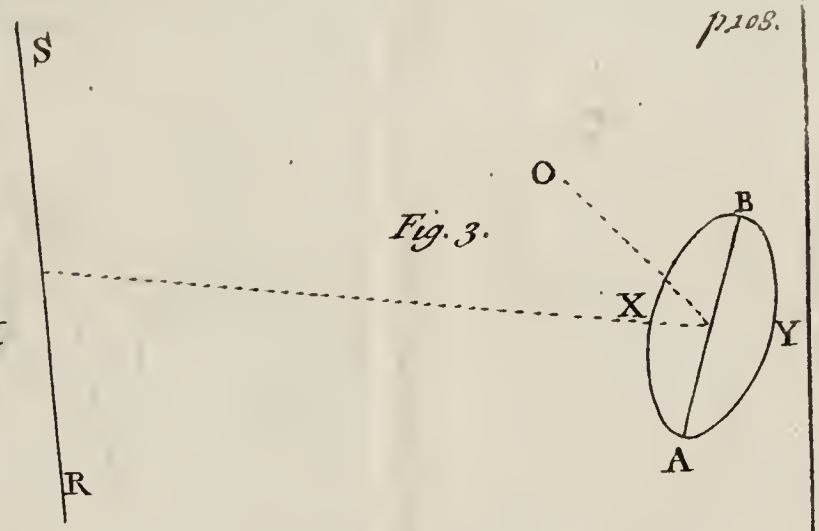


Fig. 3.

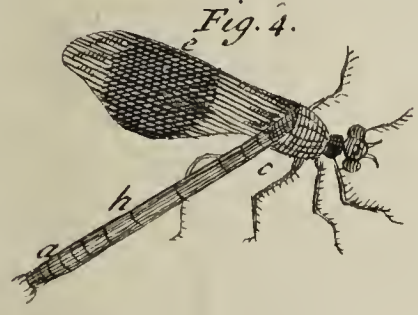


Fig. 4.

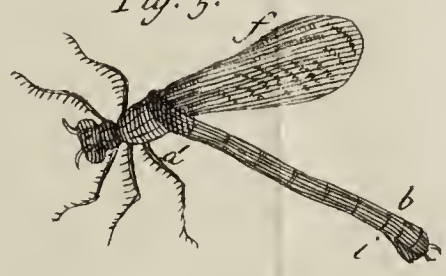


Fig. 5.

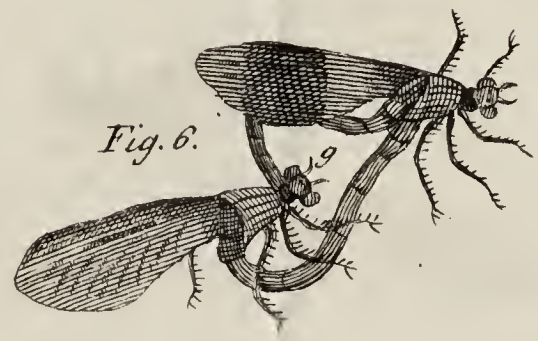


Fig. 6.

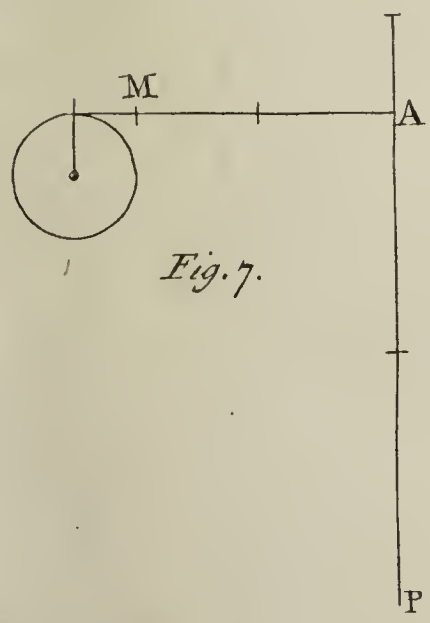


Fig. 7.

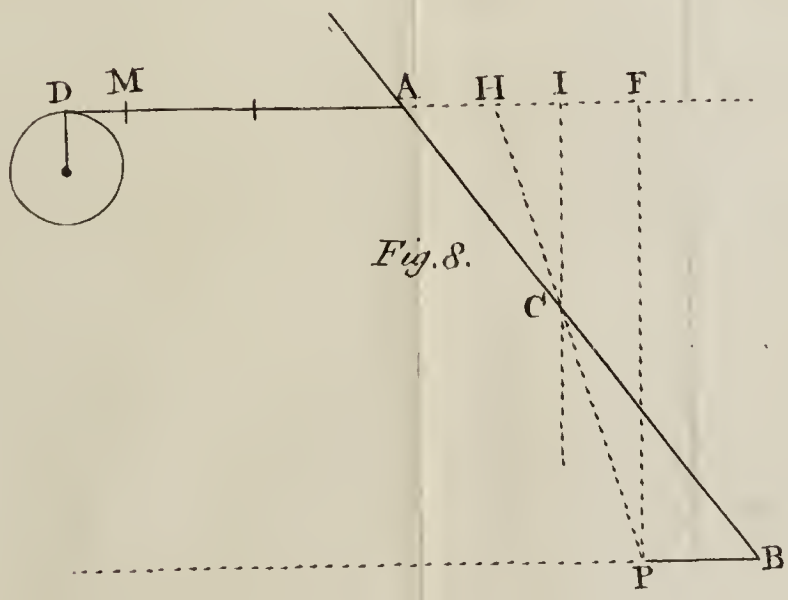


Fig. 8.

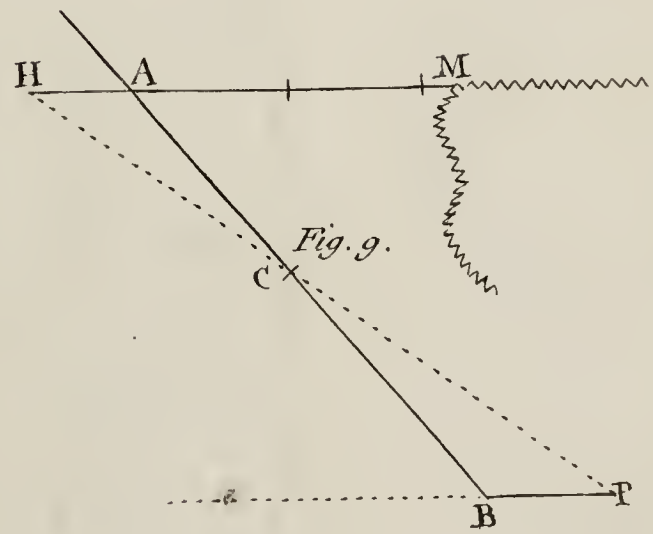


Fig. 9.

J. Mynd fc.

their weight, as that of men does, but principally on the muſcles of their body, and on the general diſpoſition of its parts, which have a very great advantage in puſhing forwards, we muſt be contented with the common experiment, that a horſe draws horizontally as much as ſeven men; and thus a horſe can draw horizontally only a little leſs than 200 *lb.* Not but that, when loaded, he can draw a little more; but it is but little in proportion to our idea of the great ſtrength of this animal. But as it is uſually conſidered as being applied to ſome wheel-carriage, ſuch as carts, we cannot make ſuch a juſt eſtimation of it, becauſe on a ſmooth and horizontal plain they need no more force than is neceſſary to overcome the frictions of the axle-trees.

We may obſerve again, that three men will do more than a horſe in carrying a burden up a ſteep hill; for three men loaded with 100 *lb.* each, will riſe more quickly and eaſily than a horſe loaded with 300 *lb*; which comes from the diſpoſition of the parts of the human body, which are better adapted to aſcend than thoſe of a horſe.

We ſee alſo by this demonſtration, that thoſe who have thought to obtain a very great advantage from the weight of the horſe, by applying it to a ſwipe, to move the piſtons of a pump, have not found in the execution all that they had concluded by the calculation of the weight of this animal, ſince at each ſtep it muſt be obliged to go up a ſort of ladder.

IX.

IX. *Two ſorts of wheels to draw water, by M.* des Billettes *.

The firſt. It is quite cloſed on one ſide by a circle of eight feet in diameter, made of planks 1 inch ½ thick, faſtened on a croſs of eight pieces of wood, which in the centre of their aſſemblage leave a hole eight inches ſquare, in which the end of an *arbor* of like bigneſs is inſerted, which turning round makes the wheel turn with it. The oppoſite ſide is void from the centre to about 13 inches from the circumference. This ſpace of 13 inches is occupied by 14 buckets nailed on the planks, being on the inſide 15 inches long, 14 high, and 11 broad. The length is underſtood of the ſpace, which they poſſeſs on the turn of the circumference, the height of their perpendicular elevation on the plane, and the breadth of their ſpace from the circumference to the centre. The outer plank of their length is cut ſome inches, and conſequently leaves an opening to give entrance to the water, every time each bucket immerges, all the other five ſides being exactly cloſed. That which covers their height conſiſts of two pieces; one of which is immoveable; the other is faſtened by two pair of hinges; ſo that it can open and ſhut like the lid of a box. On this lid is alſo faſtened a latch, made alſo like the common latches of doors, and which being preſſed by a ſpring placed on the ſame lid, engages itſelf under an iron catch faſtened to the plank on the ſide of the bucket towards the centre, or the void of the wheel. And thus the water, which has entered the bucket by its aperture, remains ſhut up as long as the latch holds the lid. But as

* December 9, 1699.

ſoon

ſoon as it unlatches, the weight of the water makes it immediately diſgorge into a trough underneath. This trough is a foot high, and two broad, and has an iron hook ſtrongly fixed, called the *decrochoir*; becauſe, as the wheel turns, this hook catches at the latches of the buckets ſucceſſively, and forcing their ſpring, diſengages or unhooks them, from below the catches which held the lids cloſe down. Then as the wheel continues to immerge, the lids ſhut, both becauſe of the loſs of their own weight, and becauſe of the water which ſtrikes them on the outſide, and gives liberty to the play of the ſprings, which hook the latches under the catches again.

The ſecond: It is leſs compound than the other. For it is formed of two hoops which cloſe it parallelly, and contain between them an arbitrary number of *volets*, cloſed at the bottom, ſides, and ends: but quite open at the circumference of the wheel. What is moſt particular, is a curve-trough, which embraces both its planes by a ſegment of a circle of about 100 degrees, ſo that below it riſes a little above the horizontal ſpoke of the wheel, and conſequently conducts all the water that it draws to a place above, where it has a ſpout, at which the water diſgorges itſelf into a gutter prepared for the voidance.

The advantage of the ſecond wheel above the other is its having a little more ſimplicity in its conſtruction; but it has alſo a great deal more friction, becauſe the trough muſt touch the ſides exactly, or elſe there will be a great loſs of water. And this friction is much more conſiderable, when towards the end of the drawing, the water having ſunk very conſiderably, the wheels muſt be inclined to the horizon. But the firſt has neither friction nor loſs of water, and ſo is in

all

all poins the beſt. With both of them one may draw 5000 *muids* of water in an hour.

X. *Of the reſiſtance cauſed in machines, both by the frictions of the parts of which they are compoſed, and by the ſtiffneſs of the cords employed in them, by M.* Amontons *.

Experiment.

We put upon ſome planes † AA, of copper, iron, lead, and wood, anointed with old lard, other planes BB of like materials, and different bigneſſes: they were preſſed one upon another differently by ſprings, like that repreſented by CCC, of which the quantity of preſſure was known. Theſe planes were changed all poſſible ways, putting ſometimes thoſe of iron on thoſe of copper, lead, and wood, and ſometimes theſe laſt on thoſe of iron; and at each time we obſerved with a ſpring balance D, the quantities of force neceſſary to make them move, and we found,

1. That the reſiſtance cauſed by the friction increaſes and diminiſhes only in proportion to the greater or leſs preſſures, according as the parts which rub have more or leſs extent.

2. That the reſiſtance cauſed by the frictions is nearly the ſame in the iron, the copper, the lead, and the wood, let them be varied how you will, when theſe ſubſtances are anointed with old lard.

3. That this reſiſtance is nearly equal to $\frac{1}{3}$ of the preſſure. To theſe remarks we may add a fourth, that theſe reſiſtances are between themſelves in a *ratio* compounded of the weights or preſſures of the parts which rub, of the times and of the velocities of their motion.

* Dec. 19, 1699. † Plate VII. Fig. 1.

For

For if, for inſtance, the plane * A is preſſed on the horizontal plane BBBB by a quantity equal to 30 *lb.* according to the third remark it muſt have a power equal to 10 *lb.* to move it, whether this plane is moved according to any right line whatſoever as AD, or according to a circular line as AE; for we may ſuppoſe this plane A ſo ſmall, that the determination about the centre C does not produce an effect which ſenſibly differs from that which it would produce according to the determination of a right line. Now it is evident, that if inſtead of applying the power at A, or any place of the *radius* CA, we apply it at F, or any place of the *radius* FC, as far diſtant from the point C, as the place where this power ſhould have been applied towards A or towards F, provided it is at an equal diſtance from theſe points, or from the centre C. But as it is demonſtrated in mechanicks, that two forces do not act equally, but when they are between themſelves in a reciprocal *ratio* from the centre of ſupport, it follows that if the power, which being applied at A was equal to the reſiſtance cauſed by the friction of the plane A, was 10 *lb.* it muſt be 20 *lb.* equal to the reſiſtance, when this power is applied at G, ſo that GC is to AC as one to two: on the contrary, ſuppoſing the reſiſtance at G, and the power at A, this power needs be no more than 5 *lb.* to equal this reſiſtance; and if both at A and G, we ſuppoſe a preſſure equal to that of the plane A, the power which ſhall be applied at A or F, needs be no more than 15 *lb.* to equal theſe two reſiſtances cauſed by the two preſſures, each of 30 *lb.* whence we ſee that theſe reſiſtances are between themſelves, when the preſſures are equal, in proportion to the ſpaces run

* Fig. 2.

thro' at the ſame time, and conſequently that they are alſo between themſelves in an inverſe *ratio* of the times of their motions, when the ſpaces run thro' are equal; whence it follows, that if a plane is doubly preſſed, and runs thro' a double ſpace in half the time that another plane does, the friction of the firſt will equal eight times that of the laſt.

It is by reaſoning according to the above principles, that we know that the friction in the ſledge makes a reſiſtance at leaſt equal to $\frac{1}{3}$ of the weight of the ſledge, and of the burden with which it is loaded; that in the waggon, this reſiſtance is leſs than in the ſledge, according to the *ratio* of the circumference of the wheel to the hole of the nave which receives the axle; and that if this *ratio* is as 18 to 1, the reſiſtance cauſed by the friction, is in the waggon equal to $\frac{1}{54}$ part of the weight of the body of the waggon, and of the burden with which it is loaded.

It is in ſhort by theſe principles, that we know why in all machines, and in all weights which are moved horizontally on a pivot, the friction is ſo inconſiderable, as well as in ballances, let the weight be ever ſo heavy; for the *ratio* of the bigneſs of that part of the pivot which rubs, to the length of the lever by which the power acts, is ſo ſmall, that it is almoſt inſenſible.

But tho' all the experiments above related ſeem to prove ſufficiently, that the reſiſtance cauſed by the friction of the ſurfaces which rub, increaſes or diminiſhes according to the greater or leſs preſſures, and not according to the greater or leſs extent of theſe ſurfaces; as this does not always ſuffice to convince a reaſonable mind, it is good however to eſtabliſh this truth by demonſtrating it.

Now,

Now if we meditate carefully on the nature of friction, we ſhall find it is nothing but *the action by which a body which is preſſed againſt another, is moved on the ſurface of that which it touches*; and that as the ſurfaces, which rub againſt one another, cannot be conſidered either as rugged and unequal, or as perfectly ſmooth, and as it is impoſſible in the firſt caſe that theſe inequalities ſhould be partly convex and partly concave, and that the firſt entring into the laſt they ſhould not produce a certain reſiſtance when we would make them move, ſince to do that they muſt remove what preſſes them againſt one another, and that the action of theſe inequalities or otherwiſe the effect which they may produce, is the ſame with that of the inclined planes, which are uſed to raiſe burdens; it follows, that the greater the preſſure, the more conſiderable is the reſiſtance to the motion: beſides, in the caſe before us, we muſt ſuppoſe the preſſure to be equally diſtributed in the whole extent of the ſurfaces: it follows alſo, that of different ſurfaces of different extents loaded with equal weights, each of the parts which compoſe the great ones is leſs loaded than each of the parts of the ſame extent which compoſe the ſmall ones, and this according to the *ratio* between their ſurfaces, and that ſo, for example, if a ſurface of a foot ſquare or 144 inches, is loaded with a weight of 144 *lb.* each ſquare inch will be loaded only with 1 *lb.* whereas the ſame weight of 144 *lb.* being ſupported by a ſurface of $\frac{1}{4}$ foot, or 36 ſquare inches, each ſquare inch will ſupport 4 *lb.* and as it is the ſame thing to raiſe to a certain height in a certain time 36 times 4 *lb.* or to raiſe in the ſame time 144 *l.* to the ſame height, it follows alſo that the reſiſtance cauſed by the friction of the ſurfaces of different extents is

always the ſame when they are loaded with equal weights, or, which is the ſame thing, when the preſſures are equal; and as when the preſſures are unequal, the forces which they muſt have to raiſe different weights to the ſame height in a certain time are between themſelves as theſe weights or theſe preſſures, it follows alſo, that the reſiſtances cauſed by different preſſures are among themſelves as theſe preſſures.

Firſt remark.

This demonſtration always ſubſiſts, whether theſe inequalities are ſuppoſed rigid, or whether they are ſuppoſed capable of elaſticity, ſince the power which would ſurmount the ſtiffneſs of a ſpring, and would make it move, for example, from * A to B, does not differ from that which would raiſe to an equal height, a weight equal to the force of this ſpring.

Second remark.

But if we ſuppoſe that the ſurfaces which rub are without any inequalities, and conſider them as mere mathematical planes, we ſhall find alſo, that this propoſition is true, ſince how eaſy ſo ever the lateral motion may be of heavy bodies, which are not removed from the centre of the earth, the laws of motion inform us, that the more gravity theſe bodies have, the more reſiſtance they make againſt being moved; add, that it is not true abſolutely ſpeaking, that two mathematical planes can be moved upon one another, in any ſituation that they are put, without one of them removing from the centre of the earth more or leſs, according to the ſituation of theſe planes, and that ſo the greater the weights are with which

* Fig. 3.

they

they are loaded, the more force they require to move them, which has no need of demonſtration.

Third remark.

Tho' it has juſt been demonſtrated, that the reſiſtance cauſed by the frictions augments according to the preſſure, and not according to the extent of the ſurfaces which rub, here is however a very particular caſe; in which, if we attend ſufficiently to it, it appears to be the quite contrary. Let there be as many planes as you pleaſe, * AAAA, preſſed between others BBBB, by the weight C taken at will. If the planes as AAAA, can be drawn all together by the ſame power D, without the planes BBBB being able to move otherwiſe, than to tranſport the preſſure of the weight C to all the planes AA and BB, ſuppoſing beſides all theſe planes without any gravity, and that we know the reſiſtance cauſed by the friction of one of the planes A, againſt one of the planes B, by the preſſure of the weight C; the power at D, which will ſurmount the reſiſtance cauſed by the friction of all theſe planes, will be to the weight C multiplied by the number of all the planes AA BB, *minus* 1, as the reſiſtance cauſed by the friction of 2 planes A and B, preſſed by the weight C, is to the ſame weight C; and if the particular reſiſtance cauſed by the friction of 2 of theſe planes, is, for inſtance, as 1 to 1, let the number of the planes be 11, the power at D muſt be decuple of the weight C, whence it follows, that a very little preſſure may make a reſiſtance, greater and greater to infinity, by augmenting alſo the number of the planes which rub againſt one another; which ſeems at firſt a meer paradox, but the truth of

* Fig. 4.

it

it is eaſily known, by conſidering that if to overcome the particular reſiſtance cauſed by the friction of 2 planes as A and B, the power, which draws one of the planes, muſt raiſe the weight C by a certain quantity, then the power, which ſhall make a greater number of theſe planes move, muſt raiſe this weight by a quantity, double, triple, quadruple, and ſo theſe powers muſt be among themſelves as the heights to which they raiſe the weight. Now as it is the ſame thing to raiſe a certain weight to a height double, triple, quadruple of another height, or to raiſe the double, the triple, the quadruple of this weight to this height at the ſame time, it follows that the great elevation of the weight C, in the caſe in queſtion, ſerves inſtead of gravity, and ſo that it is always true to ſay, that the reſiſtance cauſed by the friction, changes in proportion to the greater or leſs preſſures, and not according to the extent of the ſurfaces that rub.

What we have juſt ſaid of the great reſiſtance cauſed by ſeveral planes engaged one in another, tho' preſſed by a very ſmall weight, may ſerve exceedingly well to explain the cauſe of the hardneſs of the bodies called hard, and by oppoſition that of the fluidity, or liquidity of thoſe which are called fluid or liquid; but we reſerve the diſcourſing on it for another time.

After having ſufficiently eſtabliſhed what friction is, its nature and its laws, there remains only to ſay ſomething of the rules by which we may reduce it to calculation, to know the quantity of it in the moſt compound machines.

Firſt rule.

In machines where there are ſeveral frictions, we muſt examine them in order one after another beginning

beginning with the neareſt to the moving force, comparing the firſt with the moving force, and afterwards all the reſt with the firſt, to know the value of each in particular.

This order is ſo much the more natural, as the parts, which tranſmit the motion to the reſt, are nearer to the moving force, and as there is no friction, where there is no motion, as has been already obſerved.

Second rule.

We ſhall have the value of the firſt friction of a machine, by comparing the ſpace run thro' by the part which rubs, to the ſpace run thro' by the moving force, in the ſame time, and taking the ſuitable proportional part in the $\frac{2}{3}$ of the moving force.

We ſhall eaſily conceive the reaſon of this, if we conſider, that in the experiment of the frictions above related, the power was immediately applied to the part which cauſed the friction, and that the ſpaces run thro' by this part, and by the power in the ſame times, were conſequently equal; and that in the calculation of a friction, we muſt neceſſarily have regard to this circumſtance; it being very evident, that if the ſpace run thro' by the part which rubs, is for inſtance, but $\frac{1}{2}$ of that which ſhall be run thro' by the power in the ſame time, the friction alſo will be but $\frac{1}{2}$ of what it ſhould have been, if it had run thro' an equal ſpace, by this axiom, that every effect is proportioned to the cauſe from which it reſults; and that ſo a friction by a ſpace $\frac{1}{2}$ leſs than another in the ſame time, is an effect $\frac{1}{2}$ leſs than the other, which has been alſo demonſtrated in another manner, in conſequence of the 4th maxim of the above mentioned experiment. Now by the third

maxim

maxim of this experiment, the friction of the parts which rub is equal to $\frac{1}{3}$ of their preſſure, and this preſſure in the part of a machine the neareſt to the moving force, being always double of this force, becauſe the reſiſtance makes a like effort with the moving force upon this part, it follows that the firſt friction of a machine is always equal to $\frac{2}{3}$ of this moving force, when the ſpace run thro' by it, is equal to the ſpace run thro' by the part which rubs in the ſame time; and that this friction is leſs than the $\frac{2}{3}$ of this force, in proportion as the firſt ſpace is leſs than the ſecond, and on the contrary; and that according to the *ratio* of the ſpaces run thro' by both in the ſame times.

Third rule.

We ſhall have the value of the moving force, when only the reſiſtance is given, by calculating and comparing according to the principles of mechanicks, the ſpace which this force has to run thro' by the diſpoſition of the machine, with the ſpace which the weight, or the reſiſting force ought to run thro' in the ſame time; upon which it is to be obſerved, that when the reſiſting force, or the moving force is a weight, the ſpace, which they muſt run thro', is always meaſur'd by a plumb line, whereas, when there are other powers, this ſpace is meaſured according to the determination of the motion of theſe powers.

Fourth rule.

We ſhall have the total frictions of a machine, when after having compared each friction with the firſt and neareſt to the moving force, we add all theſe particular frictions in a ſum; but we muſt not expect that in augmenting the moving

force by a quantity equal to this value, it ſhould be ſufficient to ſurmount the reſiſting force; for this addition to the moving force immediately produces a new friction, of which we muſt find the value, and afterwards that of the augmentation of it, and that till this quantity of friction proceeding from the augmentation made each time to the power is ſo ſmall that we muſt have no farther regard to it; for example, if the reſiſting force was 100, the moving force 64, the total value of the frictions 16, this addition would produce alſo a new friction, the value of which would be 4, and upon this another friction, the value of which would be 1, if in order to ſurmount the reſiſting force, and all the frictions of the machine, the moving force ought to be equal to 64 *plus* 16, *plus* 4, *plus* 1, that is, equal to 85 and *plus*.

Experiment on the ſtiffneſs of cords.

We faſtened to a fixt body, as to the floor of a chamber, the extremities * A, A of two cords AC, AC, 5 or 6 inches diſtant from each other; the ends of theſe cords hanging freely at the bottom, bore the baſon D of a ballance.

We engaged in theſe cords a cylinder of wood BB, making each cord take a turn the ſame way about each end of the cylinder in the manner repreſented in *Fig.* 6. we afterwards put at † D a pretty conderable weight, and twiſted about the middle of the cylinder, the contrary way to the cord ‡ AEFG, that is, the way EGF, a very flexible tape, at the end of which was another little ſcale hanging freely at H, in which we put a ſufficient quantity of weights, to make the cylinder BB deſcend notwithſtanding the reſiſtance cauſed by the ſtiffneſs of the cords AC, AC.

* Fig. 5. † Fig. 5. ‡ Fig. 6.

We made these experiments with cylinders and cords of different bignesses, charged with different weights, and after having reduced the action of the weight H to an equal distance from the point of contact E, in all the cylinders, having regard to the weight of each cylinder, and of the basons H and D, we found that at $\frac{1}{2}$ inch distance from the point E,

45 ounces surmounted the resistance of two cords, each three lines diameter, loaded with a weight of 20 *lb.* and turned about a a cylinder of $\frac{1}{2}$ inch ; that

90 ounces surmounted this resistance, the weight being 40 *lb.* and

135 ounces, the weight being 60 *lb.*

Whence it follows, that the resistance caused by the stiffness of the cords about the same or equal pullies, increases in proportion to the weights which hang at the ends of the cords,

Continuing the experiment, we always found that at $\frac{1}{2}$ inch from the point E,

30 ounces surmounted the resistance of two cords, each of two lines diameter, loaded at D, with a weight of 20 *lb.* and turned about the same cylinder.

15 ounces surmounted the resistance of two cords of one line diameter, loaded in like manner at D, with a weight of 20 *lb.* and turned about the same cylinder.

Whence it follows, that the resistance caused by the stiffness of the cords increases not only in proportion to the weights, which hang at the extremities of these cords, but also in proportion to the thickness of these cords, and that so upon equal pullies these resistances are between themselves in a *ratio*, compounded of those of the weights and of the thickness of the cords.

It

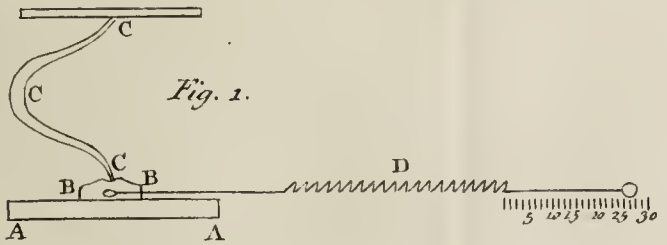


Fig. 1.

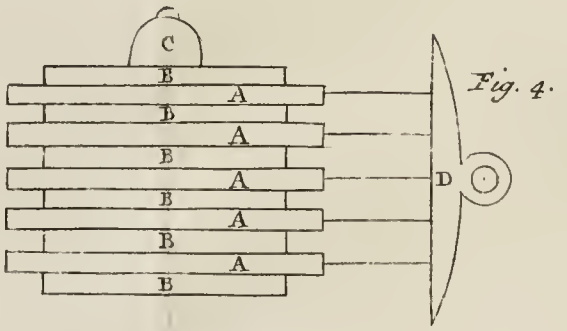
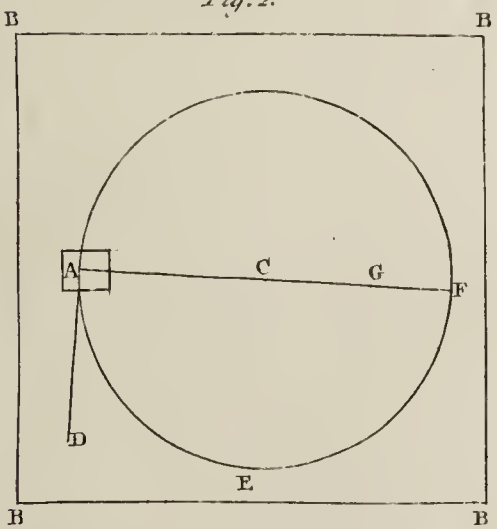


Fig. 4.

Fig. 2.



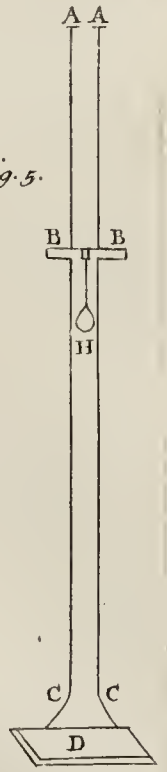
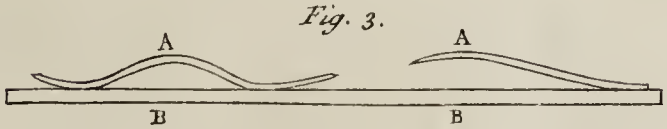


Fig. 5.

Fig. 6.

Fig. 3.



It muſt be obſerved, that the reſiſtance cauſed by the thickneſs of the cords, proceeds only from this thickneſs removing or bringing nearer the action of the weights to the point of ſupport, and not from their containing more or leſs matter; for if it was ſo, theſe reſiſtances would increaſe or diminiſh according to the ſquares of the diameters.

Continuing the experiment, we always found that at $\frac{1}{2}$ inch from the point E,

90 ounces ſurmounted the reſiſtance of two cords of three lines diameter, loaded at D with a weight of 60 *lb.* and turned about a cylinder 1 inch $\frac{1}{2}$ in diameter; that

114 ounces ſurmounted this reſiſtance with the ſame weights and cords, turned about a cylinder of one inch in diameter, and

135 ounces, the cords turned about a cylinder $\frac{1}{2}$ inch in diameter.

Whence it follows, that the reſiſtance cauſed by the ſtiffneſs of cords of equal bigneſs, loaded with equal weights, increaſes as the diameter of the pullies, about which they are turned, diminiſhes, but not according to the ſame proportion; for in the caſe now under conſideration, tho' the diameters of the pullies are between themſelves as the numbers 1, 2, 3, yet the reſiſtances increaſe only according to the numbers 90, 114, 135, whereas they ought to increaſe according to the numbers 90, 180, and 270, if they followed the proportion of the pullies.

We ſhall find the overplus of the experiment in the following table.

The weight with which the two cords were loaded.	*The resistance of the cords about a cylinder of* $\frac{1}{2}$ *inch diameter.*	*The resistance of the cords about a cylinder of* 1 *inch diameter.*	*The resistance of the cords about a cylinder of* 1 *inch* $\frac{1}{2}$ *diameter.*	*Thickness of the cords.*
60 *lb.*	135 ounces.	114 ounces.	90 ounces.	3 lines.
	90	76	60	2
	45	38	30	1
40 *lb.*	90	76	60	3
	60	$50\frac{2}{3}$	40	2
	30	$25\frac{1}{3}$	20	1
29 *lb.*	45	38	30	3
	30	$25\frac{1}{3}$	20	2
	15	$12\frac{2}{3}$	10	1

It

It were to be wiſhed, in order to determine well the proportion of the reſiſtance cauſed by the ſtiffneſs of cords of equal thickneſs, loaded with equal weights, about pullies of unequal bigneſs, that we had a greater number of experiments than thoſe here related; but in the mean time, in order to find theſe reſiſtances, as well as to find all the others already mentioned, we may make uſe of the following rules.

Rules for the calculation of the ſtiffneſs of cords in machines.

In the examination and compariſon which ſhall be made of the reſiſtance cauſed by the ſtiffneſs of the cords of a machine, we ſhall follow the ſame order as in the frictions, and make uſe of the 1ſt, 3d, and 4th rules related above, which ſerve equally for both, and which it would be needleſs here to repeat: but to have the firſt reſiſtance cauſed by the ſtiffneſs of the cords of a machine, we ſhall divide the moving force by 10, and multiply the quotient by the quantity of lines contained by the diameter of the cord, then we ſhall take the $\frac{1}{3}\frac{5}{2}$ of the product, if the diameter of the pully is but 6 lines, the $\frac{1}{4}\frac{9}{8}$ if it is 12, and the $\frac{1}{1}\frac{5}{6}$ if it is 18, and upwards; we ſhall divide this laſt product by the quantity of inches contained in the diameter of the pully, and the quotient of the diviſion will be what is ſought; the reaſon of which is, that the reſiſtance cauſed by the ſtiffneſs of the cords augmenting according to the *ratio* of the weights with which they are loaded, and according to that of the diameter of theſe cords, and that, according to the above experiment, the weight hanging to the extremity of a cord 1 line in diameter, being 10 *lb.* when the re-

reſiſtance is 7 inches $\frac{1}{2}$ on a cylinder of $\frac{1}{2}$ an inch, 6 ounces $\frac{1}{2}$ on a cylinder of one inch, and only 5 ounces of a cylinder of 1 inch $\frac{1}{2}$ and upwards, it follows that dividing by this weight of 10 *lb.* the weight equal to the moving force, we have the *ratio* of this weight to the weight of the experiment, and afterwards the *ratio* compounded of theſe weights and of the thickneſſes of the cords, by multiplying the quotient by the number of lines contained by the diameter of the cord; ſo that the product of this multiplication expreſſes the augmentation of the reſiſtance cauſed by the ſtiffneſs of the cords, which are above a line in diameter, and are loaded with more than 10 *lb.* And as by the above experiment this reſiſtance is 7 ounces $\frac{1}{2}$, on a pully of $\frac{1}{2}$ inch, 6 ounces on a pully of 1 inch, and only 5 ounces on a pully of 1 inch $\frac{1}{2}$ and upwards; it follows, that according to the diameter of the pully, we need only multiply either of theſe quantities by this product, or, which is the ſame thing, take either the $\frac{1}{3}\frac{5}{2}$ or the $\frac{1}{4}\frac{5}{8}$ or the $\frac{5}{1\,6}$ of it, to have the quantity of this reſiſtance; but as in this experiment the diſtance of the action of this reſiſtance from the point of ſupport, or the length of the lever by which it reſiſts is $\frac{1}{2}$ inch, it is proper alſo to have regard to that of the pully; and if its *radius* or lever is more than $\frac{1}{2}$ inch, that is, if the pully is more than an inch in diameter; divide alſo the laſt product by the number of $\frac{1}{2}$ inches contained in the ſemidiameter, or, which is the ſame, by the number of inches contained in the whole diameter.

But if neither the diviſion of the weight equal to the moving force, by 10 *lb.* which is the leaſt weight, as a line is the leaſt thickneſs of the cord, in the experiment which ſerves for a foundation to this calculation, nor the multiplication of the

the quotient by the lines of the diameter of the cord have place, becauſe both of them, or one only may be leſs, we ſhould need only to take the proportionate part of the leaſt reſiſtance which is found in the table of the above experiment; for example, if we would know the reſiſtance cauſed by a cord of a line in diameter, and loaded only with $\frac{1}{3}$ of 10 *lb.* we need but take $\frac{1}{3}$ or 7 ounces $\frac{1}{2}$, if the diameter of the pully was $\frac{1}{2}$ inch, or $\frac{1}{3}$ of 6 ounces $\frac{1}{2}$ of this diameter was 1 inch, or $\frac{1}{3}$ only of 5 ounces, if this diameter was 1 inch $\frac{1}{2}$ and upwards; having alſo regard to the diameter of the pullies, that is, dividing alſo the quotient by the number of inches contained in the diameter of the pully about which the cord ſhould be thrown; and if the weight, not only was leſs than 10 *lb.* but alſo the cord was leſs than a line in diameter, we muſt alſo, after having taken the proportional part for the weight as above, without having regard to the diminution of the cord, take alſo from the quotient another proportional part for the diminution of the cord: for example, if the cord being loaded only with 3 *lb.* $\frac{1}{3}$ was but $\frac{1}{2}$ line thick, after having taken $\frac{1}{3}$ or 7 ounces $\frac{1}{2}$, or 6 ounces $\frac{1}{3}$, or 5 ounces as before, we muſt alſo take the $\frac{1}{2}$ of one or other of theſe quantities, and divide it by the number of inches, contained in the diameter of the pully, to have the reſiſtance cauſed by the ſtiffneſs of this cord about this pully.

To facilitate the calculation of the reſiſtance cauſed by the ſtiffneſs of the cords in machines, I have drawn up the following table, wherein you may find theſe reſiſtances for all ſizes of cords from 1 line to 30 lines, loaded with all ſorts of weights from 1 *lb.* to 100,000.

The uſe of the following table.

Seek in the margin of this table the diameter or bigneſs of the cord expreſſed in lines, and at the top of the table the weights ſupported by this cord; of what you ſhall find under each weight, over-againſt the diameter of the cord, make a ſum, and divide it by the quantity of inches contained in the diameter of the pully; the quotient of the diviſion will be the reſiſtance cauſed by the ſtiffneſs of this cord.

Example.

Seek the reſiſtance cauſed by the ſtiffneſs of a cord of 18 lines in diameter, loaded with a weight of 12393 *lb.* and paſſing about a pully 2 inches in diameter.

For 10,000 *lb.* you will find over-againſt 18 lines	5625	
2,000	1125	
300	168	12
90	50	10
3	1	11
All theſe ſums added together make	6971	1

Which being divided by 2 inches diameter of the pully give 3485 *lb.* 8 ounces ½ for the reſiſtance ſought.

Moreover, when the ſame cord is not equally flexible throughout, we muſt not expect that the calculation will always agree exactly with experience, this not being morally poſſible, but only very near: we may ſay as much of the frictions, becauſe the different conſiſtences of the greaſe, which

which is more or leſs thick, and the parts which rub intercepting more or leſs, make theſe reſiſtances vary; this does not hinder the uſe of theſe rules from ſhewing pretty exactly the effect to be expected from a machine, to reckon certainly upon it, this extreme exactneſs being otherwiſe of no uſe; and we may make uſe of the preſent, till more ample experiments give us room to eſtabliſh ſome that are more certain.

The uſe of pullies being very common, it will not be improper, before I finiſh this diſcourſe, and to make trial of our rules, to compare the reſiſtances cauſed, both by the friction of the rundles, and by the ſtiffneſs of the cords in pullies of different bigneſſes; and the rather becauſe thoſe, who do not uſually attend to theſe reſiſtances, will ſee by the calculation, that it is not ſo indifferent a thing as they imagine to prefer ſmall pullies to great ones, becauſe of the ſmall ſpace that they occupy. Let it be propoſed therefore to raiſe a load of 800 *lb.* with a pully of 24 inches, or with one of 3 inches only: in the firſt place, it is evident, that in both theſe pullies the rundles muſt be of equal ſtrength, as well as the cords, ſince they have an equal weight to ſupport: let the rundle therefore of each pully be one inch in diameter, and the cord 20 lines; we ſhall find by making the calculation according to the above rules, that in the pulley of 24 inches, beſides the

800 *lb.* which is wanted to equal the weight of the load propoſed to be raiſed, the moving force muſt alſo be augmented by that of

21 *lb.* to ſurmount the reſiſtance cauſed by the ſtiffneſs of the cord, by that of

22 *lb.* to ſurmount the reſiſtance cauſed by the friction of the rundle, and alſo by that of

1 *lb.* to ſurmount the reſiſtance cauſed by the ſtiffneſs of the cord, becauſe of the augmentation of 21 *lb.* and 22 *lb.* and laſtly by that of

1 *lb.* to ſurmount the reſiſtance cauſed by the friction of the rundle, becauſe of the ſaid augmentation of 21 *lb.* and 22 *lb.* all theſe weights together making that of

845 *lb.* ſo that a moving force ſuperior to this weight will raiſe with the pully of 24 inches diameter, the weight propoſed of 800 *lb.*

But with the pulley of 3 inches, the ſame calculation ſhews us, that beſides the weight of 800 *lb.* neceſſary to equal the load, there muſt alſo be that of

167 *lb.* to ſurmount the reſiſtance cauſed by the ſtiffneſs of the cord; that of

178 *lb.* to ſurmount the reſiſtance cauſed by the friction of the rundle; that of

37 *lb.* to ſurmount the reſiſtance cauſed by the ſtiffneſs of the cord, becauſe of the augmentation of the 167 *lb.* and 178 *lb.* that of

38 *lb.* $\frac{1}{3}$ to ſurmount the reſiſtance cauſed by the friction of the rundle, becauſe of the ſaid augmentation; that of

8 *lb.* to ſurmount the reſiſtance cauſed by the ſtiffneſs of the cord, becauſe of the augmentation of the 37 *lb.* and 38 *lb.* $\frac{1}{3}$; and laſtly, that of

8 *lb.* $\frac{1}{3}$ to ſurmount the reſiſtance cauſed by the friction of the rundle, becauſe of the augmentation of the ſaid 37 *lb.* and 38 *lb.* $\frac{1}{3}$.

All which weights together make that of 1236 *lb.* $\frac{2}{3}$, that is, 391 *lb.* $\frac{2}{3}$ more than with the pully of 24 inches, in which the cord and the rundle do not make a reſiſtance equal to $\frac{1}{16}$ of the burden, whereas in that of 3 inches the ſame reſiſtance is more than $\frac{1}{2}$ the ſame load.

Weights ſupported by the cords.

Diameters of the cords.	lb. 1	lb. 2	lb. 3	lb. 4	lb. 5	lb. 6	lb. 7	lb. 8	lb. 9	lb. 10
	oun. dr	l. oun.	l. oun. dr.	l. oun.	l. oun. dr.	l. oun.	l. oun. dr.	l. oun.	l. oun. dr.	l. oun.
1	4	1	1 4	2	2 4	3	3 4	4	4 4	5
2	1 0	2	3 0	4	5 0	6	7 0	8	9 0	10
3	1 4	3	4 4	6	7 4	9	10 4	12	13 4	15
4	2 0	4	6 0	8	10 0	12	14 0	1 0	1 2 0	1 4
5	2 4	5	7 4	10	12 4	15	1 1 4	1 4	1 6 4	1 9
6	3 0	6	9 0	12	15 0	1 2	1 5 0	1 8	1 11 0	1 14
7	3 4	7	10 4	14	1 1 4	1 5	1 8 4	1 12	1 15 4	2 3
8	4 0	8	12 0	1 0	1 4 0	1 8	1 12 0	2 0	2 4 0	2 8
9	4 4	9	13 4	1 2	1 6 4	1 11	1 15 4	2 4	2 8 4	2 13
10	5 0	10	15 0	1 4	1 9 0	1 14	2 3 0	2 8	2 13 0	3 2
11	5 4	11	1 0 4	1 6	1 11 4	2 1	2 6 4	2 12	3 1 4	3 7
12	6 0	12	1 2 0	1 8	1 14 0	2 4	2 10 0	3 0	3 6 0	3 12
13	6 4	13	1 3 4	1 10	2 0 4	2 7	2 13 4	3 4	3 10 4	4 1
14	7 0	14	1 5 0	1 12	2 3 0	2 10	3 1 0	3 8	3 15 0	4 6
15	7 4	15	1 6 4	1 14	2 5 4	2 13	3 4 4	3 12	4 3 4	4 11
16	8 0	1 0	1 8 0	2 0	2 8 0	3 0	3 8 0	4 0	4 8 0	5 0
17	8 4	1 1	1 9 4	2 2	2 10 4	3 3	3 11 4	4 4	4 12 4	5 5
18	9 0	1 2	1 11 0	2 4	2 13 0	3 6	3 15 0	4 8	5 1 0	5 10
19	9 4	1 3	1 12 4	2 6	2 15 4	3 9	4 2 4	4 12	5 5 4	5 15
20	10 0	1 4	1 14 0	2 8	3 2 0	3 12	4 6 0	5 0	5 10 0	6 4
21	10 4	1 5	1 15 4	2 10	3 4 4	3 15	4 9 4	5 4	5 14 4	6 9
22	11 0	1 6	2 1 0	2 12	3 7 0	4 2	4 13 0	5 8	6 3 0	6 14
23	11 4	1 7	2 2 4	2 14	3 9 4	4 5	5 0 4	5 12	6 7 4	7 3
24	12 0	1 8	2 4 0	3 0	3 12 0	4 8	5 4 0	6 0	6 12 0	7 8
25	12 4	1 9	2 5 4	3 2	3 14 4	4 11	5 7 4	6 4	7 0 4	7 13
26	13 0	1 10	2 6 0	3 4	4 1 0	4 14	5 11 0	6 8	7 5 0	8 2
27	13 4	1 11	2 7 4	3 6	4 3 4	5 1	5 14 4	6 12	7 9 4	8 7
28	14 0	1 12	2 9 0	3 8	4 6 0	5 4	6 2 0	7 0	7 14 0	8 12
29	14 4	1 13	2 10 4	3 10	4 8 4	5 7	6 5 4	7 4	8 2 4	9 1
30	15 0	1 14	2 12 0	3 12	4 11 0	5 10	6 9 0	7 8	8 7 0	9 6

Weights supported by the cords.

Diameters of the cords.	lb. 10	lb. 20	lb. 30	lb. 40	lb. 50	lb. 60	lb. 70	lb. 80	lb. 90	lb. 100
	l. oun.	l. oun.	l. oun.	l. oun.	l. oun.	l. oun.	l. oun.	l. oun.	l. oun.	l. oun.
1	5	10	15	1 4	1 9	1 14	2 3	2 8	2 13	3 2
2	10	1 4	1 14	2 8	3 2	3 12	4 6	5 0	5 10	6 4
3	15	1 14	2 13	3 12	4 11	5 10	6 9	7 8	8 7	9 6
4	1 4	2 8	3 12	5 0	6 4	7 8	8 12	10 0	11 4	12 8
5	1 9	3 2	4 11	6 4	7 13	9 6	10 15	12 8	14 1	15 10
6	1 14	3 12	5 10	7 8	9 6	11 4	13 2	15 0	16 14	18 12
7	2 3	4 6	6 9	8 12	10 15	13 2	15 5	17 8	19 11	21 14
8	2 8	5 0	7 8	10 0	12 8	15 0	17 8	20 0	22 8	25 0
9	2 13	5 10	8 7	11 4	14 1	16 14	19 11	22 8	25 5	28 2
10	3 2	6 4	9 6	12 8	15 10	18 12	21 14	25 0	28 2	31 4
11	3 7	6 14	10 5	13 12	17 3	20 10	24 1	27 8	30 15	34 6
12	3 12	7 8	11 4	15 0	18 12	22 8	26 4	30 0	32 12	37 8
13	4 1	8 2	12 3	16 4	20 5	24 6	28 7	32 8	36 9	40 10
14	4 6	8 12	13 2	17 8	21 14	26 4	30 10	35 0	39 6	43 12
15	4 11	9 6	14 1	18 12	23 7	28 2	32 13	37 8	42 3	46 14
16	5 0	10 0	15 0	20 0	25 0	30 0	35 0	40 0	45 0	50 0
17	5 5	10 10	15 15	21 4	26 9	31 14	37 3	42 8	47 13	53 2
18	5 10	11 4	16 14	22 8	28 2	33 12	39 6	45 0	50 10	56 4
19	5 15	11 14	17 13	23 12	29 11	35 10	41 9	47 8	57 7	59 6
20	6 4	12 8	18 12	25 0	31 4	37 8	43 12	50 0	56 4	62 8
21	6 9	13 2	19 11	26 4	32 12	39 6	45 15	52 8	59 1	65 10
22	6 14	13 12	20 10	27 8	34 6	41 4	48 2	55 0	61 14	68 12
23	7 3	14 6	21 9	28 12	35 15	43 2	50 5	57 8	64 11	71 14
24	7 8	15 0	22 8	30 0	37 8	45 0	52 8	60 0	67 8	75 0
25	7 13	15 10	23 7	31 4	39 1	44 14	54 11	62 8	70 5	78 2
26	8 2	16 4	24 6	32 8	40 10	46 12	56 14	65 0	73 2	81 4
27	8 7	16 14	25 5	33 12	42 3	48 10	59 1	67 8	75 15	84 6
28	8 12	17 8	26 4	35 0	43 12	50 8	61 4	70 0	78 12	87 8
29	9 1	18 2	27 3	36 4	45 5	52 6	63 7	72 8	81 9	90 10
30	9 6	18 12	28 2	37 8	46 14	54 4	65 10	75 0	84 6	95 12

Weights supported by the cords.

Diameters of the cords.	lb. 100	lb. 200	lb. 300	lb. 400	lb. 500	lb. 600	lb. 700	lb. 800	lb. 900	lb. 1000
	l. oun.	l. oun.	l. oun.	l. oun.	l. oun.	l. oun.	l. oun.	l.	l. oun.	l. oun.
1	3 2									
2	6 4	12 8	18 12	25 0						
3	9 6	18 12	28 2	37 8	46 14	56 4	65 10	75	84 6	
4	12 8	25 0	37 8	50 0	62 8	75 0	87 8	100	112 8	125 0
5	15 10	31 4	46 14	62 8	78 2	93 12	109 6	125	140 10	156 4
6	18 12	37 8	56 4	75 0	93 12	112 8	131 4	150	168 12	187 8
7	21 14	43 12	65 10	87 8	109 6	131 4	153 2	175	196 14	218 12
8	25 0	50 0	75 0	100 0	125 0	150 0	175 0	200	225 0	250 0
9	28 2	56 4	84 6	112 8	140 10	168 12	196 14	225	253 2	281 4
10	31 4	62 8	93 12	125 0	156 4	187 8	218 12	250	281 4	312 8
11	34 6	68 12	103 2	137 8	171 14	206 4	240 10	275	309 6	343 12
12	37 8	75 0	112 8	150 0	187 8	225 0	262 8	300	337 8	375 0
13	40 10	81 4	121 14	162 8	203 2	243 12	284 6	325	365 10	406 4
14	43 12	87 8	131 4	175 0	218 12	262 8	306 4	350	393 12	437 8
15	46 14	93 12	140 10	187 8	234 6	281 4	328 2	375	421 14	468 12
16	50 0	100 0	150 0	200 0	250 0	300 0	350 0	400	450 0	500 0
17	53 2	106 4	159 6	212 8	265 10	318 12	371 14	425	478 2	531 4
18	56 4	112 8	168 12	225 0	281 4	337 8	393 12	450	506 4	562 8
19	59 6	118 12	178 2	237 8	296 14	356 4	415 10	475	534 6	593 12
20	62 8	125 0	187 8	250 0	312 8	375 0	437 8	500	562 8	625 0
21	65 10	131 4	196 14	262 8	328 2	393 12	459 6	525	590 10	656 4
22	68 12	137 8	206 4	275 0	353 12	412 8	481 4	550	618 12	687 8
23	71 14	143 12	215 10	287 8	369 6	431 4	503 2	575	646 14	718 12
24	75 0	150 0	225 0	300 0	385 0	450 0	525 0	600	675 0	750 0
25	78 2	156 4	234 6	312 8	400 10	468 12	546 14	625	703 2	781 4
26	81 4	162 8	243 12	325 0	416 4	487 8	568 12	650	731 4	812 8
27	84 6	168 12	253 2	337 8	431 14	506 4	590 10	675	759 6	844 12
28	87 8	175 0	262 8	350 0	447 8	515 0	612 8	700	787 8	876 0
29	90 10	181 4	271 14	362 8	463 2	533 12	634 6	725	815 10	907 4
30	95 12	187 8	281 4	375 0	478 12	552 8	656 4	750	843 12	938 8

Weights supported by the cords.

Diameters of the cords.	lb. 1000		lb. 2000		lb. 3000		lb. 4000	lb. 5000	lb. 6000		lb. 7000	lb. 8000	lb. 9000		lb. 10000	
	l.	oun.	l.	oun.	l.	oun.	l.	l.	oun.	l.	oun.	l.	l.	oun.	l.	oun.
1																
2																
3																
4	125	0	250	0												
5	156	4	312	8	468	12										
6	187	8	375	0	562	8	750									
7	218	12	437	8	656	4	875	1093	12							
8	250	0	500	0	750	0	1000	1250	0	1500	0					
9	281	4	562	8	843	12	1125	1406	4	1687	8	1968	12	2250		
10	312	8	625	0	937	8	1250	1562	8	1875	0	2187	8	2500	2812	8
11	343	12	687	8	1031	4	1375	1718	12	2062	8	2406	4	2750	3093	12
12	375	0	750	0	1125	0	1500	1875	0	2250	0	2625	0	3000	3375	0
13	406	4	812	8	1218	12	1625	2031	4	2437	8	2843	12	3250	3656	4
14	437	8	875	0	1312	8	1750	2187	8	2625	0	3062	8	3500	3937	8
15	468	12	937	8	1406	4	1875	2343	12	2812	8	3281	4	3750	4218	12
16	500	0	1000	0	1500	0	2000	2500	0	3000	0	3500	0	4000	4500	0
17	531	4	1062	8	1593	12	2125	2656	4	3187	8	3718	12	4250	4781	4
18	562	8	1125	0	1687	8	2250	2812	8	3375	0	3937	8	4500	5062	8
19	593	12	1187	8	1781	4	2375	2968	12	3562	8	4156	4	4750	5343	12
20	625	0	1250	0	1875	0	2500	3125	0	3750	0	4375	0	5000	5625	0
21	656	4	1312	8	1968	12	2625	3281	4	3937	8	4593	12	5250	5906	4
22	687	8	1375	0	2062	8	2750	3337	8	4125	0	4812	8	5500	6187	8
23	718	12	1437	8	2156	4	2875	3493	12	4312	8	5031	4	5750	6468	12
24	750	0	1500	0	2249	0	3000	3650	0	4500	0	5250	0	6000	6750	0
25	781	4	1562	8	2342	12	3125	3806	4	4687	8	5468	12	6250	7031	4
26	812	8	1625	0	2436	8	3250	3962	8	4875	0	5687	8	6500	7312	8
27	844	12	1687	8	2530	4	3375	4118	12	5062	8	5906	4	6750	7593	12
28	876	0	1750	0	2624	0	3500	4275	0	5250	0	6125	0	7000	7874	0
29	907	4	1812	8	2717	12	3625	4431	4	5437	8	6343	12	7250	8155	4
30	938	8	1874	0	2811	8	3750	4587	8	5625	0	6562	8	7500	8436	8

Weights supported by the cords.

Diameters of the cords.	lb. 10000		lb. 20000	lb. 30000		lb. 40000	lb. 50000		lb. 60000	lb. 70000		lb. 80000	lb. 90000		lb. 100000
	l.	oun.	l.	l.	oun.	l.	l.	oun.	l.	l.	oun.	l.	l.	oun.	l.
1															
2															
3															
4															
5															
6															
7															
8															
9															
10	3125	0													
11	3437	8													
12	3750	0	7500												
13	4062	8	8125												
14	4375	0	8750												
15	4687	8	9375	14072	8										
16	5000	0	10000	15000	0										
17	5312	8	10625	15937	8										
18	5825	0	11250	16875	0	22500									
19	5937	8	11875	17812	8	23750									
20	6250	0	12500	18750	0	25000									
21	6562	8	13125	19687	8	26250	32812	8							
22	6875	0	13750	20624	0	27500	34375	0							
23	7187	8	14375	21561	8	28750	35937	8	42125						
24	7500	0	15000	22498	0	30000	37500	0	44000						
25	7812	8	15625	23435	8	31250	39062	8	45875	54687	8				
26	8135	0	16250	24473	0	32500	40624	0	47750	56875	0				
27	8447	8	16875	25410	8	33750	42186	8	49625	59062	8	67500			
28	8759	0	17500	26348	0	35000	44749	0	51500	61250	0	70000			
29	9071	8	18125	27285	8	36250	46311	8	53375	63437	8	72500	81562	8	
30	9384	0	18750	28223	0	37500	47874	0	55250	65625	0	75000	84385	0	93750

XI. *A deſcription of the heart of the tortoiſe, and of ſome other animals, by* M. du Verney *.

Part. I. Sect. I. *The ſtructure of the heart of the tortoiſe.*

Before I opened the heart of the great tortoiſe, I obſerved, that the ſcale, which cover'd it, was 2 feet, 3 inches long, to 2 feet 1 inch broad, and its under ſcale 1 foot 5 inches long, to 1 foot 2 inches broad; whereas the upper ſcale of our common tortoiſes is but 6 or 7 inches long to 5 ½ broad, and the under one 3 inches ½ broad, to 5 or 6 inches long.

The *pericardium* of theſe animals is a membrane of a very cloſe texture. Thro' all its circumference, it is cloſely united with the *peritonæum*, and its capacity is very large in proportion to the bulk of the heart †.

This heart is ſituated in the upper part of the *thorax*, above the liver: there is no *mediaſtinum*. In the ſmall tortoiſes, there is a ligament, which goes from the *apex* of the heart, and is faſtened to the bottom of the *pericardium*; there is none in the *American* tortoiſe. This ligament is a prolongation of the membrane which wraps the fibres of the heart ‡.

The figure of the heart of the great tortoiſe is hemiſpherical; its lower part being convex, and the upper plain, but a little dented in the middle, which is the place where the auricles and arteries are implanted, ſo that this heart pretty much reſembles the kidney of a ſheep; but in our ſmall tortoiſes it is drawn out a little into a point ‖.

* *Dec.* 23. 1699.
† Plate VIII. Fig. 1.
‡ Fig. 3.
‖ Fig. 2 and 3.

In the great tortoiſe, the heart being meaſured from the middle of the *baſis* to the *apex* is 1 inch 5 lines; and about 3 inches from one of the ſides of the *baſis* to the other; but in one of the parts, the diſtance from the middle of the *baſis* to the *apex* is but 6 lines, and 9 lines from one of the ſides of the *baſis* to the other.

We ſee under the right auricle of the heart of theſe animals, a ſort of reſervoir of an oblong figure, and pretty like that of a blown up bladder; it is formed by the concourſe of ſeveral veins. The right axillary, and the *vena cava inferior* open into the right ſide of this reſervoir, one at top and the other at bottom. On the other ſide we ſee in a like ſituation the left axillary, and a vein which returns the blood from the left part of the liver. The coronary vein and ſome other veſſels, which proceed from the neighbouring parts, empty themſelves alſo into it; and as the jugulars diſcharge themſelves into the axillaries, this cauſes the blood of all the veins to be brought back into this reſervoir, except that of the veins of the lungs*.

This reſervoir is furniſhed on the inſide with ſeveral fleſhy fibres, which croſs and interlace one another, almoſt like thoſe on the inſide of the auricles of the human heart. All the veins, which ſerve to form this reſervoir, are alſo furniſhed with fibres, which are interlaced in the ſame manner†.

This reſervoir alſo towards its middle, opens into the right auricle, on the ſide towards the upper ſcale ‖.

The 2 pulmonary veins riſe along the lower ſide of each branch of the *trachea*; the right,

* Plate IX. Fig. 3. and Plate X. Fig. 2.
† Plate IX. Fig. 4. ‖ Fig. 1 and 2.

having

having run thro' the *pericardium*, paſſes behind the reſervoir, and advances to the left auricle. The left pulmonary vein having alſo run thro' the *pericardium*, hides itſelf behind the axillary on the ſame ſide, and unites with the right pulmonary, to the poſterior part of the left auricle, near its neck, where they form a ſort of reſervoir*.

At the opening of the great reſervoir into the right auricle, there are 2 valves ſituated ſomething obliquely with regard to the right auricle. They reſemble 2 eye-lids, and are compoſed of fleſhy fibres produced by thoſe of the auricle. At their exterior angle, they are faſtened by a bundle of fibres, which riſing a little obliquely towards the bottom of the auricle, expand themſelves and are loſt in it.

The lower valve has a little more extent than the upper one, and when they join, they ſhut this aperture exactly †.

The baſon of the little reſervoir, is alſo furniſhed on the inſide with fleſhy fibres, but in leſs quantity than that of the great reſervoir. In the ſmall tortoiſes (which I have not ſeen in the great one) there is at its outlet a fleſhy valve in form of a half moon, turned ſo that the angles are towards the right auricle. It is like that which is found in birds that have the opening of the trunk of the pulmonary vein into the auricle ‖.

The auricles are of different bigneſs; the right has a greater capacity than the left: they reſemble two purſes, which lying ſide by ſide, are joined together at their aperture, that is, at the ſhorteſt part; they are ſeparated from each other at this place by a membrane: this ſeparation is a little

* Fig. 1 and 5. † Plate XI. Fig. 1.
‖ Plate IX. Fig. 2. and Plate VIII. Fig. 3.

leaning towards the middle of the *baſis* of the heart, but more to the left ſide than the right.

This membrane is covered in its greateſt part with fleſhy fibres, which are continuations of thoſe of the auricles: the *baſis* of it is quite tendinous, and ſo thin that you may ſee thro' it; it is faſtened between the 2 valves, which are at the outlets of the auricles, and of which we ſhall ſpeak afterwards *.

The bottom of theſe auricles is ſpherical, and much bigger than the place where they join: they contract themſelves towards the *baſis* of the heart, and form a ſort of neck, which is divided into two by the membranous partition ſpoken of above †.

All the inſide of theſe auricles is furniſhed with muſcular fibres, which are interlaced ſo many different ways, that they compoſe, eſpecially in the ſmall tortoiſes, a very ſpongy texture; which makes them, when ſwoln and dried, in ſome meaſure reſemble the ſubſtance of the lungs: the membrane, which ſeparates them, is quite ſmooth ‖.

At the outlet of each auricle there is a valve, and theſe 2 valves are joined together by one of their extremities, and covered with fleſhy fibres, directed from one ſide of the *baſis* of the heart to the other. The membrane between the auricles, is faſtened on the outſide to the middle of theſe 2 valves, as has been ſaid, and thus makes the ſeparation in this place. By the ſide, which they have at liberty, they may, by falling and becoming concave, let the blood paſs from the auricles into the heart, and on the other they hinder the return of it by riſing. Thus we ſee, that tho' theſe 2 valves joined together make a ſort of

* Plate VIII. Fig. 2. Plate X. Fig. 3. and Plate XI. Fig. 1 and 2. † Plate X. Fig. 2 and 3. ‖ Plate XI. Fig. 1.

long

long ſquare, when they are applied againſt the apertures of the auricles, yet, when the blood paſſes thither, each repreſents ſeparately the quarter of a concave oval, oppoſed and joined to another by the ſummit*.

Theſe valves have this in common with that of the *foramen ovale* of the *fœtus*, that, when they open the paſſage to the blood, they become hollow like gutters, and when they cloſe and apply themſelves againſt the hole, they become flat. Two things alſo contribute to apply them againſt the hole: the firſt is the impulſe of the blood; the ſecond is the action of the fleſhy fibres, which compoſe them: theſe fibres from crooked becoming ſtrait, and the blood which ſtrikes againſt theſe valves thus flatted, and applied againſt their holes, maintaining them in this ſtate.

There are 3 cavities in the heart of this animal, one is in the right ſide of the part which I call the anterior, which is that towards the under ſcale; the 2 others occupy the part which I call the poſterior, which is towards the upper ſcale†.

Of the 2 cavities in the poſterior part of the heart, I ſhall hereafter call the firſt cavity, that which receives the blood from the right auricle; the ſecond cavity, that which occupies the whole left part, and receives the blood from the left auricle; and the third cavity, that which is towards the right ſide of the anterior part of the heart, and into which the pulmonary artery opens.

The texture of the inſide of the heart, is furniſhed with fleſhy columns of different ſizes, interlaced with each other.

There are ſeveral, which riſing from the middle of the poſterior face, up to the *baſis* on the

* Plate XI. Fig. 1. and 5. † Fig, 1, and 2.

ſame

ſame ſide, leave under the valves of the auricles a paſſage, by which the firſt and ſecond cavity communicate together; ſo that we may truly ſay it is but one. As the firſt cavity communicates likewiſe with the third, we muſt alſo ſay that all the 3 make but one; the blood which is emptied into the heart by the right and left auricle, being able to mix eaſily, and to enter from one cavity into the other.

The valve of the outlet of the right auricle is ſo diſpoſed, that the blood which paſſes into it flowing from left to right tends only to fill the firſt cavity; which communicating with the third, the blood enters at the ſame time: but the valve of the left auricle being turned from right to left, the blood, which comes from it, fills at firſt only the ſecond cavity *.

We have ſaid that the texture of fleſhy fibres, which ſeparates the firſt cavity from the ſecond, leaves a paſſage for the blood to go from one to the other. This paſſage is of the ſame length with the *baſis* of the valves, and is about 3 lines in diameter; ſo that the valves being depreſſed, there always remains an aperture, and the communication of the firſt and ſecond cavity is not entirely hindred by it *.

It muſt be obſerved in this place, that the communication of the firſt cavity with the third, is made by an arched aperture, compoſed of fleſhy fibres about 2 lines in diameter. This aperture is on the right ſide of the anterior face of the heart, near the outlet of the artery, which I ſhall call the left branch of the deſcending *aorta* *.

The ſurface of the heart appears all over of an equal thickneſs: thence it comes that there being 2 cavities one upon the other in its right part,

* Plate XI. Fig 5.

their

their ſides are leſs thick than thoſe of the ſecond cavity; but the ſides of the firſt are alſo thicker than thoſe of the third *.

This heart is compoſed of ſeveral orders of fibres; the exterior ones deſcribe curve lines, and appear to be directed from left to right; the interior ones form ſeveral columns, as has been ſaid, whence ariſe theſe ſorts of partitions, which ſeparate the cavities of the heart; and being interlaced various ways leave as it were ſo many little cells, which communicate with each other; which makes the heart when ſwoln and dry to appear all over ſpongy.

Towards the right part of the anterior face of the *baſis* of the heart, there proceed 3 conſiderable arteries: 2 of theſe arteries compoſe the *aorta*, and open into the firſt cavity of the heart. The orifices of this are placed between the outlet of the right auricle, and the aperture which makes the communication of the firſt cavity with the third, and they are ſo near as to touch one another †.

The third artery, which is that of the lungs, proceeds immediately from the third cavity of the heart †.

The outlets of theſe 3 veſſels are ſupported by an almoſt ſemicircular cartilage, to which alſo their valves are faſtened; and at each of theſe outlets there are 2 ſigmoidal valves, which have the ſame uſe as in other animals ‡.

Theſe arteries are cloſely united; two of them are almoſt in front, namely that which I call the left branch of the deſcending *aorta*, and the pul-

* Fig. 1 and 2. † Plate VIII. Fig. 2. Plate IX. Fig. 1. Plate X. Fig. 1. and Plate XI. Fig. 5.
‡ Fig 2, 3, and 4.

monary

monary artery, and behind theſe is that which I call the firſt trunk of the *aorta**.

In our ſmall land tortoiſes, theſe arteries are embraced at their origin by a ring of fleſhy fibres: there was none in the heart of the *American* tortoiſe †.

Theſe 3 veſſels, after having riſen to a certain height, are bent back again.

This artery, which makes the firſt trunk of the *aorta*, ſoon after its origin divides into 2 branches, one of which riſes, and diſtributes itſelf to the fore parts, and to all the upper parts, I ſhall call it the aſcending *aorta*; the other deſcends bending under the ventricle, without making any branch, and joins itſelf to the left branch of the deſcending *aorta*, I ſhall call it the right branch of the deſcending *aorta* ‡.

The left branch of rhe deſcending *aorta* bends alſo on the left ſide of the heart, and without making any branch, it deſcends alſo under the ventricle, and in this place furniſhes 2 arteries, the ſuperior of which is in the room of the cœliac, and the inferior in the room of the meſenteric; afterwards it is united with the right branch of the deſcending *aorta*, and theſe 2 branches thus re-united form the trunk of the deſcending *aorta*, which is diſtributed into the parts of the lower belly, and all the lower extremities ‡.

The pulmonary artery, which immediately touches the left branch of the deſcending *aorta*, ariſes, as has been ſaid, from the third cavity of the heart: this artery is very big, and is as much in diameter as the firſt trunk of the *aorta*: after having riſen a little, it divides into two branches,

* Plate X. Fig. 1. † Plate VIII. Fig. 3.
‡ Fig. 2. and Plate IX. Fig. 1.

which

which are bent alſo, one of which goes to the left part of the lungs, and the other to the right *.

The coronary artery is compoſed of one ſingle trunk, which proceeds from the firſt trunk of the *aorta*, a little above the ſigmoid valves, and it is diſtributed to the heart and auricles ‖.

Sect. II. *The ſtructure of the heart of the frog.*

The heart of the frog is of a conical figure, like that of moſt animals, and incloſed in a *pericardium*, which has leſs capacity in proportion, than that of the tortoiſe.

There is under this heart a ſort of round reſervoir, about three lines in diameter.

The *vena cava inferior*, proceeding from the liver, receives a great vein on each ſide; one belonging to the right and the other to the left part of this bowel.

The concourſe of theſe three veſſels ſerves principally to form this reſervoir, which receives the axillaries on each ſide: the coronary vein is alſo diſcharged into it †.

This reſervoir opens above and towards the right ſide of the auricle: its outlet is furniſhed with two valves, ſituated obliquely with regard to the auricle: they form together as it were two eye-lids: the upper is larger than the lower, as well as in the tortoiſe, and they have the ſame uſe ‡.

To each part of the lungs towards the back, is faſtened the trunk of the vein which returns the blood from it; both theſe veins, in quitting the lungs, proceed about two lines, inclining towards each other; and uniting form but one

* Plate VIII. Fig. 2, Plate IX. Fig. 1. and Plate X. Fig. 1. ‖ Plate X. Fig. 3 and 4. † Plate XII. Fig. 1. ‡ Fig 2.

trunk, which is ſtuck to the upper part of the reſervoir. This trunk alſo opens into the auricle immediately above the outlet of the reſervoir into the auricle ‖.

There is but one auricle in the heart of this: it is ſpherical, and it not only covers the whole baſis of the heart, but has a great deal more extent.

Its outlet is very large, and furniſhed with two valves, one of which is faſtened to that part of the *baſis* of the heart towards the ſpine, and the other to the oppoſite part of the ſame *baſis*: they are ſemicircular, ſhaped almoſt like thoſe which we call ſigmoid; ſo that the blood being driven upwards in the contraction of the heart ſwells them, and brings the two edges exactly together *.

In this heart there is only one ventricle, and the ſides of it are very thin; the fleſhy fibres of its inner part interlace in ſo many different manners, that they compoſe a very ſpongy texture.

There is alſo but one artery which proceeds immediately from the right *baſis* of the heart: this veſſel from its origin lies on the right ſide of the auricle, of which it covers almoſt $\frac{1}{3}$; and it goes obliquely to the length of about two or three lines; and afterwards divides into two branches, one of which goes to the right, and the other to the left, bending a little: each of theſe branches is ſubdivided into three others †.

Of theſe three the upper one towards the head is divided into two branches, one of which is diſtributed to all the parts under the throat, and the other riſes to the brain.

‖ Plate XII. Fig. 3. * Fig. 4. † Fig. 5.

The

The branch of the middle one, which is the biggeſt, bends, and forms the deſcending *aorta*. Over-againſt the arm-pit it is divided into two great branches, the ſmalleſt of which is diſtributed to the muſcles under the ſhoulder, upon the back and on the head; the other, which is much bigger, forms the axillary. Afterwards this branch deſcends below the heart, throwing off ſeveral branches, which go to the ſpine; and uniting with that on the oppoſite ſide, they form but one trunk, whence proceeds a great branch, which is inſtead of the cœliac and meſenteric: this trunk is afterwards diſtributed to all the lower parts.

The third branch, which is the lower, divides alſo into two others, the ſmalleſt of which is divided into two branches, which diſtribute themſelves among the muſcles, ſerving for the motions of the head: the other branch is divided into three or four conſiderable branches, which at their origin are faſtened to the lungs, and make an infinite number of other ſmall branches, which communicate together, and embrace on all ſides above and below *.

This *aorta* has two ſigmoidal valves at its opening into the heart; and, as we have ſaid, it riſes obliquely the ſpace of about three lines, and in this place it is covered again with circular fleſhy fibres. Within this veſſel, there is in the middle a cartilaginous *lamina*, faſtened only to the part of the canal towards the ſpine, and turned ſpirally from left to right.

This *lamina* is terminated in a ſigmoidal valve, and on the ſide there are two others of the ſame figure, each ſuſtained by a little cartilaginous button: they are all three ſituated at the place where

* Plate XII Fig. 5.

this veſſel begins to divide; and when they are raiſed, they cloſe the entrance very exactly †.

This mechaniſm, which is very ſingular, has however a great ſimilitude with that of the *aorta* of ſome fiſhes. For in the ſkate, and ſeveral others, the *aorta*, at its origin, is alſo cloathed with very thick circular fleſhy fibres, to the length of about an inch; and there appear on the inſide four rows of cartilaginous tubercles, between which the blood flows which comes out of the heart: each of theſe tubercles ſupports a valve, and theſe valves being open, hinder the return of the blood: thoſe of the moſt diſtant row from the heart are the biggeſt. The figure of theſe tubercles is ſo irregular, and the bigneſs of the valves ſo different, that it would be tedious to enumerate them particularly *.

Sect. III. *The ſtructure of the heart of the viper.*

The heart of the viper is formed like that of the frog; but is more flat, and the left ſide is more elevated than the right: it is placed at about ſix inches diſtance from the head, and one inch above the liver ‖.

The *pericardium* is of a great capacity, and ſo thin, that the heart may eaſily be ſeen thro' it. There are three *venæ cavæ*, one inferior and two ſuperior. The *vena cava inferior* is the biggeſt; the *ſuperior* on the right ſide inoſculates with the *inferior*, and in this place, each of theſe veſſels is a little bent ‡.

The *ſuperior* on the left ſide paſſing above the deſcending *aorta*, ſticks to the left auricle; and making a little elbow, deſcends under the *baſis* of

† Fig. 6. * Fig. 7. ‖ Fig. 8.
‡ Fig. 9.

the

the heart, to open into the *vena cava inferior* near its outlet †.

The trunk formed by the union of these three vessels has its outlet towards the middle of the right side of the right auricle, and this outlet is furnished with two valves formed like eye-lids, as in the tortoise and frog, situated obliquely with regard to the auricle ‡.

The pulmonary vein is composed of two branches, one which returns the blood from the upper part of the lungs, and is the biggest; and the other which returns it from the lower part.

These two veins meet on the left side of the left auricle, and the trunk formed by their union goes obliquely above the lower part of the auricle, to which it is closely stuck, to open itself into the same auricle by a very oblique insertion *.

The two auricles of the heart of the viper do not differ from those of the small land-tortoise, except in being longer and narrower; they are separated by a partition of the same figure.

At the opening of each auricle into the heart, there is a valve, and these valves have exactly the same conformation with those in the heart of the tortoise.

Lastly, there are three cavities in this heart, the inner configuration of which is entirely like that of the cavities of the heart of the tortoise; and they have the same communication with each other.

Three arteries proceed from the right side of the *basis* of the heart: there are two in front, and a third behind them, and of these three, there are two likewise, which form the *aorta* ‖.

The first trunk of the *aorta* rises to the height of four or five lines, and is divided into two

† Fig. 10. ‡ Fig. 11. * Fig. 10. ‖ Fig. 8.

branches,

branches, of which the biggeſt that is on the right ſide bends, and deſcending about three lines, throws off a branch, which makes the right carotid; it then deſcends, and having furniſhed ſome branches to the ſpine, it receives the *aorta*, which is on the oppoſite ſide. The other branch produces the left carotid. We may call this firſt trunk of the *aorta*, as in the tortoiſe, the aſcending *aorta*, becauſe it furniſhes blood to all the upper parts ‖.

The ſecond trunk of the *aorta* riſes to the height of about three lines, croſſing the two others: it bends afterwards and deſcends about 1 inch $\frac{1}{2}$ below the heart, to unite itſelf with the *aorta* on the oppoſite ſide. In deſcending it throws off a branch, which goes to the ventricle. This branch of the artery may alſo be called the deſcending *aorta*, becauſe it diſtributes itſelf only to the lower parts *.

The third artery, which is that of the lungs, riſes to the height of four lines: it lies on the middle part of the lungs, and is there divided into two branches: this diviſion is immediately under the aſcending *aorta*.

The branch which is intended for the upper part of the lungs, is much bigger than the other; and accompanies the vein thro' its whole extent ‡.

Sect. IV. *The ſtructure of the heart of fiſhes.*

Being about to deſcribe the heart of fiſhes, I have made choice of the carp, becauſe it is eaſily procured.

The heart of this fiſh is ſituated below the *fauces*, which are above the ears, at the bottom of the mouth. The cavity, in which the heart is incloſed, is covered with a very ſmooth mem-

‖ Fig 8. * Fig 8. ‡ Fig. 8.

brane,

brane, which is in the place of the *pericardium* in ſeveral other fiſhes, but cannot be called ſo here, becauſe the heart is alſo incloſed in a very thin pellicle, which is properly its *pericardium* ‡.

The *baſis* of this cavity is cloſed by a membrane, which ſeparates the heart from all the other *viſcera*, and is a continuation of the preceding.

Under the heart there is a reſervoir formed by the concourſe of ſeveral veins, three of which proceed from the liver, and ſerve only to return the blood from the *vena portæ*, and from one part of the ovaries: of theſe three there are two, which open on each ſide into the bottom of this reſervoir, and the third diſcharges itſelf into it alſo by a very large outlet. Two other veins riſe on each ſide of the ſpine, accompanying the *aorta*; and unite an each ſide of the reſervoir with the veins which proceed from the ſides of the liver: thus theſe two veſſels have on each ſide, in this place, but one outlet. The trunk of the vein, which returns the blood from the gills, lies above the *aorta*: it deſcends on the right ſide of the heart, it is ſtuck to the ſides of the cavity where the heart is incloſed, and making a turn, it opens into the right ſide of the reſervoir *.

This reſervoir opens above towards the middle of the lower part of the auricle: at its outlet it has two valves, in form of eye-lids, like thoſe of the animals already deſcribed, and thoſe which are at the outlet of the *vena cava inferior* of birds, of which we ſhall ſay nothing, becauſe it is ſo foreign to the ſubject we are treating on †.

This heart has but one auricle, but of a great capacity. It is applied to the left ſide; and in its upper part it forms on each ſide a proceſs or horn,

‡ Plate XIII. Fig. 1. * Fig. 2. † Fig. 3.

horn, of which the left is bigger than the right: its outlet is in the upper part of the left ſide of the heart *.

There are two valves at the opening of the auricle into the heart, the one above, and the other below, faſtened by all the ſemicircle which they form, and open on the ſide of the *apex* of the heart; which cauſes the blood, that flows back by the contraction of the heart, to raiſe them, and join them to each other, as in the frog †.

This heart is of a ſemicircular figure, and flatted pretty much like a cheſnut: it is placed in ſuch a manner with regard to the head, that the two flat ſides are towards their gills: its *baſis* is articulated with the *aorta* by a ſort of *gynglimus*; theſe two parts having eminences and cavities, which receive each other mutually ‖.

The ſides of this heart are very thick, in proportion to its bulk, and its fibres are of a very compact texture.

To have the diſtribution of the veſſels in this fiſh well underſtood, we muſt have ſome notion of the ſtructure of the gills. Wherefore we ſhall ſay, that the gills, which are known to ſerve for lungs to fiſhes, are, as I may ſay, divided into two lobes, each of which is compoſed of four *laminæ*, placed near one another, and ſuſtained by four bony arches. We ſhall call that the firſt arch on each ſide, which is neareſt the heart.

The convex part of theſe arches is hollow like a gutter, along which the veſſels flow, which I ſhall ſpeak of afterwards. The *laminæ* ſuſtained by theſe arches occupy the whole ſpace between the *maxillæ* and the *fauces*; they are compoſed of a double row of oſſeous *lamellæ*. Each of theſe *lamellæ* is formed like a little ſcythe; and at its

* Fig. 4. † Fig. 5. ‖ Fig. 6.

origin has a little sort of foot, which is thicker than the rest, and hollow underneath in form of a gutter: this foot leans only by its extremity upon the edge of the arch, to which it is fastened only by means of the very thick membrane which covers the arch: the convex side of this *lamina* is furnished quite to the point with threads, which diminish in length as they approach this point; and the concave side has them much shorter, and is furnished with them only as far as the middle *.

These threads are bound together on each side, by a very fine bony membrane, which gathers them by the middle almost thro' their whole length; but as the extremities are not joined, they represent the teeth of a saw †.

We have said that each *lamina* is composed of a double row of *lamellæ*; we must add, that the concave of each of these *lamellæ* is applied to the convex of that which is opposite to it, and that they are all bound together by a membrane, which reaches from their origin to the middle of their height, where becoming thicker it forms a sort of ligament, above which it is fastened to the *lamellæ* by the ends of as many *lunulæ*, as there are spaces between them. The rest of the *lamellæ* is free, and ends in a very fine and supple point ‡.

The resting of these *lamellæ* on the edges of the arch, being by the extremity of their foot, as has been said, there remains in the middle a little void in form of a triangular canal, which reaches the whole length of the arch, and serves to lodge the vessels ‖.

These *lamellæ* are covered with a very fine membrane, and serve only to support the ramifications of all the vessels of the gills. These ves-

* Fig. 7, 8. † Fig. 8. ‡ Fig. 9. ‖ Fig. 10.

ſels, which run in the gutter of each arch, are an artery, a vein, and a nerve.

Before we ſpeak of the diſtribution of the arteries, we ſhall obſerve that the part of the *aortæ* which riſes from the heart, and has 2 ſigmoidal valves, like that of the tortoiſe, is of no great bulk, in proportion to that which it has a little above; for at firſt it ſpreads ſo as to cover the whole *baſis* of the heart, then contracting by degrees it forms a ſort of cone, from the point of which proceeds the veſſel which is the continuation of the *aorta*. The inſide of its dilated part is filled with ſeveral fleſhy columns, which diminiſh continually towards the top; and they have between their baſes ſome interſtices, which form cavities, where the refluent blood is received; which ſtrengthens the action of the valves juſt mentioned, and produces the ſame effect with the valves in the muſculous part of the *aorta* of the ſkate and frog *.

The canal, which comes out of the point of the cone of the *aorta*, runs between the two lobes of the gills. Over againſt the firſt pair of arches of theſe lobes, it makes on each ſide a great branch, which is ſubdivided again into 2 others, the firſt of which runs on each ſide in the gutter of this firſt pair of arches, and the ſecond in the gutter of the ſecond pair. The ſame trunk, in its courſe, divides again into 2 branches, each of which goes on its own ſide to the third pair; and more forward into 2 others, which go to the laſt pair of theſe arches.

Each artery running along the baſe of each *lamina*, throws off as many pairs of branches as there are pairs of *lamellæ*, and loſes itſelf entirely in the extremity of the *lamina*; ſo that the *aorta*

* Fig. 11.

and

and its branches run only from the heart to the extremity of the gills, where they end *. For the diſtribution of each pair of arteries, ſee *Fig.* 14, and 15.

On the edge of each *lamella* there is a vein, and each vein diſcharges itſelf into a trunk, which runs in the gutter of each arch, the ramifications of which are plainly ſeen in the figures. Theſe veins coming from the extremity of each arch, which is towards the *baſis* of the *cranium*, take the conſiſtence of arteries, and are reunited by pairs on each ſide. This, for inſtance, which comes out of the fourth arch, after having furniſhed branches, which diſtribute the blood to the organs of ſenſation, to the brain, and to all the other parts of the heads joins itſelf with that of the third arch. Thus they make but one branch: This branch, after having gone about 2 lines is united with that on the oppoſite ſide, and both form but one trunk, which running under the *baſis* of the *cranium*, receives alſo ſoon after on each ſide another branch, formed by the reunion of the veins of the ſecond and firſt pair of arches. This trunk continues its courſe along the *vertebræ*, and diſtributing the blood to all the other parts, performs the office of a deſcending *aorta*. Theſe veins, by their other extremity, which is towards the origin of the arches, diſcharge themſelves into a trunk which is inſerted into the reſervoir †.

The conformity, which is found in the ſtructure of the heart of theſe animals, has obliged us to deſcribe them at the ſame time.

But before I explain the uſes of them, it will not be amiſs to obſerve, that by the term reſervoir, I mean nothing but a trunk of veins formed

* Fig. 12, 13. † Fig. 16, 17.

by the concourſe of ſeveral others, which is inſtead of the upper and lower *vena cava* in the tortoiſe and carp; and in the frog, it is nothing but the trunk of the *vena cava inferior*, which receives the 2 axillaries; for tho' this reſervoir or trunk is furniſhed with fleſhy fibres, we do not pretend to ſay that it is not of the kind of veins, ſeeing thoſe, which open into the auricles and cavities of the heart of other animals, are alſo covered in this place with like fibres.

Part II. *The uſes of the heart of the tortoiſe.*

In the deſcription which we have made of the ſtructure of the heart of the tortoiſe, it may be obſerved, that it differs in many things from that of moſt other animals.

The firſt difference is that of the ventricles: for tho' the three cavities of the heart of the tortoiſe are ſeparated by partitions, yet there being openings of communications between them, they make properly but one ventricle; whereas there are two in men, quadrupeds, and birds; becauſe the partition between theſe ventricles ſeparates them entirely. We cannot give the cavities of the heart of the tortoiſe, the name of right and left ventricle, fixing the uſual ideas to theſe 2 words; becauſe on one ſide, if we conſider them with regard to the auricles, and the courſe of the venal blood, one of them might really be called the right ventricle, and the other the left; but if we conſider them with regard to the origin of the arteries, the ſame cavity, which we call the right ventricle, ſhould alſo be called the left ventricle, becauſe it gives riſe to the 2 arteries which ſupply the place of the *aorta*: that which we call the left ventricle would then have no arteries, and

what

what we call the third ventricle would have neither auricles nor veins, which is contrary to the conformation of the heart of man, and of moſt animals.

The ſecond difference regards the circulation of the blood in the cavities of the heart; for in men, quadrupeds and birds, all the blood, which is returned by the *vena cava*, paſſes thro' the right ventricle, and from thence into the pulmonary artery; and all that returns from the lungs enters again into the left ventricle, and from thence into the 2 arteries which ſupply the place of the *aorta*.

But in the tortoiſe, the blood which is returned from all parts of the body, except the lungs, enters into the right auricle, by the great reſervoir, which contracting itſelf by the action of the fleſhy fibres, with which it is lined on the inſide drives it into the auricle; and as the valve, which is at the opening of this auricle into the heart, is ſo diſpoſed that the blood which it drives by contracting itſelf, runs from left to right, it is manifeſt that every time the auricle empties itſelf, it fills not only the firſt cavity, but alſo the third which is only a continuation of it.

There are 2 valves in form of eye-lids, at the outlet of this reſervoir, which in the contraction of the auricle join together, and cloſing this aperture exactly, hinder the blood with which this auricle is filled, from returning into this reſervoir; which obliges it to flow entirely into the ventricle of the heart.

We find in birds ſuch like valves at the opening of the *vena cava* into the auricle; and in quadrupeds, inſtead of valves, we ſee between the 2 *venæ cavæ* and within their opening, ſome returns formed by bundles of fleſhy fibres, which

diſengage

diſengage themſelves in ſuch a manner about theſe veſſels, that they form as it were ſo many ſplinters, ſeeing they embrace not only the ſpace between the *venæ cavæ*, but alſo their outlet, and they cannot contract, without binding in a manner theſe 2 veins at their entrance into the auricle. In man theſe returns are leſs diſtinct.

We ſee by this, that theſe ſphincters and theſe valves have the ſame uſe: for as theſe valves permit the blood to enter from the reſervoir into the auricle, and hinder the return of it; in like manner theſe ſphincters being relaxed, permit the blood of the veins to fill the auricle; but when they contract, they cloſe the apertures of theſe veſſels, and hinder the return of the blood.

The blood, which is brought back by the pulmonary vein, fills the left auricle. In the ſmall tortoiſes, and in birds, there is a fleſhy valve at the outlet of this vein, which hinders the return of the blood: and afterwards the left auricle contracting, tends only to fill the ſecond cavity, becauſe of the valve being turned from right to left, which is at its outlet.

By the compreſſion of the heart, all the blood contained in the ſecond cavity is forced to enter again into the firſt, this cavity having no arteries, by which it may diſcharge itſelf. At the ſame time that the heart contracting itſelf drives the blood out of the ſecond cavity into the firſt, it drives alſo into the principal trunk of the *aorta*, and the left branch of the deſcending *aorta*, the blood which was contained in this firſt cavity, to diſtribute it into all its parts; laſtly in the time that the firſt cavity empties itſelf, the blood of the third cavity is alſo driven into the artery, which goes to the lungs, and diſtributes itſelf into their whole ſubſtance. We ſee thereby, that

theſe

theſe 3 cavities empty themſelves at the ſame time, and concur to drive the blood into the arteries.

The ring or ſphincter at the origin of the *aorta* in the ſmall tortoiſe, cloſing itſelf immediately after the contraction of the heart, gives room to believe, that its principal uſe is to accelerate and augment the motion of the blood towards the extremities.

In frogs and ſeveral fiſhes, this circular ring occupies a conſiderable part of the *aorta*; which makes us judge, that by its contraction it drives the blood alſo with more force towards all the parts of the body; and it ſeems that the valves, which are found in greater number in this part of the *aorta*, are intended to hinder the reflux of it.

The third difference is in the manner in which the blood is mixed in the cavities of the heart. In man, all the blood, which is deprived of its active parts, enters into the right ventricle, to be carried thence into the lungs, where it muſt receive all the preparations neceſſary to animate and vivify the parts; and it is afterwards carried into the left ventricle, and into the *aorta*, which diſtributes it thro' the whole body.

In the tortoiſe, at each circulation, a little more than $\frac{1}{3}$ of the blood paſſes into the lungs, where it receives all the preparations neceſſary for its uſes; and the blood, which flows into them, is principally that which is incloſed in the third cavity, and is almoſt wholly venal: the other portion of the blood of the veins, which is in the firſt cavity, mixes itſelf with that of the ſecond cavity, which is returned from the lungs; and by this mixture it is gradually impregnated with the active parts, with which the firſt was charged in the lungs, as much as is neceſſary for the occaſions of the animals; thus all the blood, which

returns from the lungs, is mixed in the cavities of the heart of the tortoiſe with that of the veins: but in the ventricle of the human heart, there is no ſuch mixture made; and all the blood, which returns from the lungs, paſſes from the left ventricle into the *aorta*.

Let us here make ſome reflections, in order to perceive the differences better, which are between the heart of the tortoiſe, and that of the other animals, of which we have ſpoken.

Three things particularly eſtabliſh this difference: the firſt is the communication between theſe cavities: the ſecond is, that the *aorta* derives its origin from the right cavity: and the third is, that the left has no arteries.

To diſcover the reaſon of this difference, we muſt obſerve, that the body of man, and of the animals which we have ſpoken of, ſuffers a diſſipation and a conſiderable loſs of ſubſtance, by all the functions performed when awake, and by the rapid motion of the blood and ſpirits; and this loſs cannot be ſufficiently repaired, unleſs the whole blood diſcharged by the *venæ cavæ* into the right ventricle circulates thro' the lungs, in order to go into the left ventricle, and from thence into the *aorta*; becauſe it is in the lungs that the air communicates to the blood ſome parts ſo active and ſo penetrating, that its heat, fluidity, and fermentation depend upon them; it is by this mixture that it is rendered fit for nouriſhment, and that it can, by circulating in the brain, furniſh it with theſe quick and ſubtile parts, called animal ſpirits, and ſerve in ſhort for all the other uſes.

We muſt not therefore be ſurprized, if man (who ſtands in need of abundance of nouriſhment, and of a prodigious quantity of ſpirits, to furniſh

ſo

ſo many different ſenſations, and all the motions of a perſon awake, which are ſo violent, and of ſo long a continuance) has need alſo, that all the blood furniſhed by each of the *venæ cavæ* ſhould circulate alſo thro' the lungs; but it is ſufficient for the tortoiſe, which ſpends the whole winter in reſt and in a ſort of numbneſs, which can alſo live ſeveral months during the greateſt heats of ſummer, incloſed in a veſſel, without taking any nouriſhment, which has but very ſlow motions, and but ſeldom pulſations of the heart, and which hardly perſpires at all: it is ſufficient, I ſay, that $\frac{1}{3}$ of the blood, which comes out of the heart, ſhould be carried into the lungs, there to receive the preparations neceſſary for the life of the animal, and that this portion of blood ſhould be mixed again, with that which is to be driven thro' the *aorta* into all the parts of the body. In frogs the 2 pulmonary veins diſcharge themſelves into the auricle: in ſalamanders they are emptied into the *vena cava inferior* near its opening into the heart: thus in all theſe animals the mixture is made, before the blood enters into the heart; but in tortoiſes, ſerpents, and vipers, the 2 pulmonary veins empty themſelves into the ſecond cavity, and thus this mixture is made in the heart. We may therefore ſay that it was neceſſary, that theſe cavities ſhould have a communication, that the blood, which returns from the lungs, might mix itſelf with that of the veins; and the *aorta* was to derive its origin from the firſt cavity, which is the place where this mixture is made, becauſe it is to diſtribute the blood impregnated with theſe active particles to the whole body. Tho' fiſhes have a great analogy to theſe animals, yet the circulation is performed in them after a different manner, ſeeing the blood, which

comes out of the heart at each pulsation, is distributed into the gills by an infinite number of small arteries, which cover the surfaces of all the *lamellæ*, of which they are composed; and since the veins, which bring back this blood, distribute it to all the parts, after the manner of arteries. The reason of this difference is, that the small quantity of air engaged between the parts of the water, which separates from it only with difficulty, and by the compression, which it receives between the *lamellæ* of the gills, must be applied to a greater surface of blood, to furnish sufficiently these active particles to the occasions of these animals.

Altho' the 3 cavities of the heart of the tortoise must be considered as one single ventricle, yet there is room to believe, that all the blood which is brought thither by the *vena cava* and pulmonary vein, is not exactly mixed there; the sorts of partitions which distinguish these cavities, hinder the perfect mixture of it; and the blood, which comes from the lungs, emptying itself by the contraction of the heart into the cavity, whence the *aortæ* derive their origin is probably determined to fill these vessels, and above all the principal trunk of the *aorta*, the aperture of which is the largest, and the most exposed to the direction of this vivified blood; and it is this, which supplies the head, and the upper parts, where there is need of a greater abundance of active parts. But the pulmonary artery being derived from the third cavity, which could be filled only with the blood of the first cavity, which is almost all venal, brings into this bowel only such blood as has been deprived of the active parts, with which it must there be impregnated.

An explanation of the figures.

Plate VIII. *Fig.* 1. repreſents the heart of the tortoiſe incloſed in its *pericardium*, and ſerves only to ſhew the great capacity of the *pericardium*, in proportion to the bulk of the heart. All that appears thro' it will be explained in the next figure.

Fig. 2. repreſents the heart, its auricles, and veſſels, in their natural bigneſs; and we have added to it a portion of the *trachea*, and of the branches of the pulmonary arteries and veins.

A. The great reſervoir formed by the concourſe of the following veins.

B. The *vena cava inferior*.

C. The right axillary.

D. The jugular on the ſame ſide.

E. A vein which brings back the blood from the left part of the liver.

F. The left axillary.

G. The jugular on the ſame ſide.

H. H. Two veins which come out of the two partition lobes.

I. The heart.

K. The right auricle.

L. The left auricle.

M. The pulmonary artery.

N. The left branch of the deſcending *aorta*; behind theſe 2 veſſels is hidden that which I call the principal trunk of the *aorta*.

O. The bend of the right branch of the deſcending *aorta*.

P. The branch which riſes a little before it makes a bend; it is divided on each ſide into 2 others, which are,

Q. The right axillary.

R. The carotid on the ſame ſide.

S. The left axillary.

T. The carotid on the ſame ſide.

b. b. Two little arteries which proceed from the carotids, and diſtribute themſelves into a gland which fills the ſpace left between them.

V. The right branch of the deſcending *aorta*, which after having made a bend deſcends into the lower belly, to unite itſelf with the *aorta* on the left ſide.

X. The left bend of the deſcending *aorta*.

Y. The ſame veſſel, which having deſcended below the ventricle throws off

d. The branch which ſerves for a cœliack.

e. That which ſerves for a meſenterick.

Z. The place where the 2 branches of the deſcending *aorta* are again united.

2. The place where the pulmonary artery goes off.

3. The right bend of this veſſel, which paſſes behind the 2 trunks of the *aorta*, and is incloſed under the bend of the deſcending *aorta*.

4. The ſame veſſel which deſcends on the exterior ſide of the *bronchia*, to be implanted at *e* into the lungs.

5. The left bend of the pulmonary artery placed under the left branch of the deſcending *aorta*.

6. The ſame veſſel which deſcends to the left ſide of the lungs.

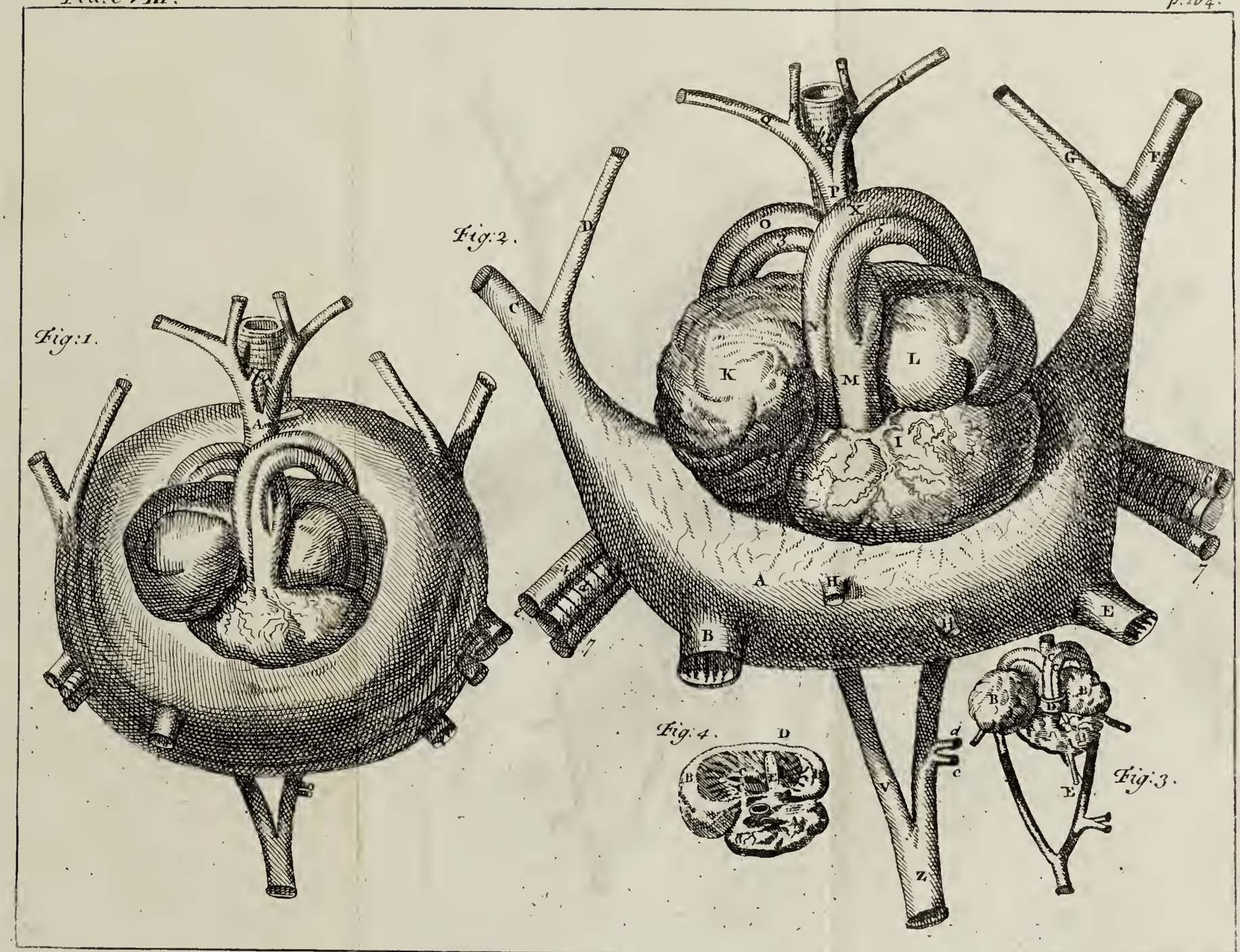
7. 7. The pulmonary veins, which riſe again to the interior ſide of the *bronchia*.

Fig. 3. repreſents the heart of a ſmall land tortoiſe.

A. The heart.

B. B. Its auricles.

C. C. The arteries with their bends.



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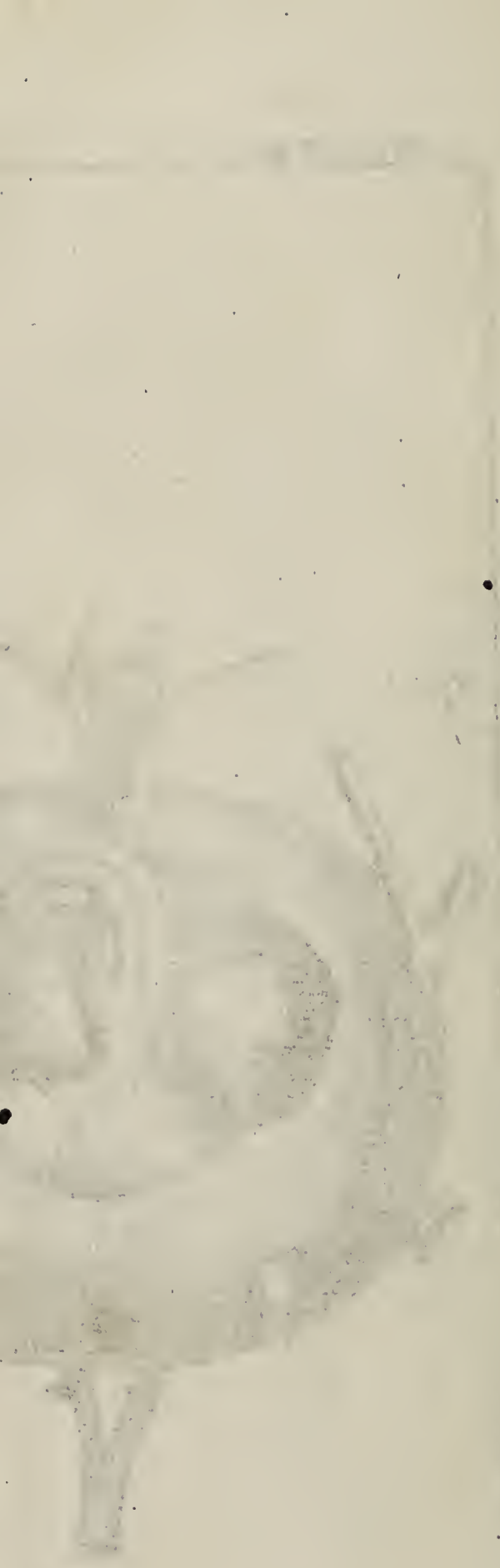


D. A ring of fleſhy fibres, which embraces theſe arteries at their coming out of the heart.

E. A ligament which parts from the *apex* of the heart, and faſtens it to the bottom of the *pericardium.*

F. A ſmall gland placed between the carotids.

Fig. 4. repreſents the 2 auricles cut from top to bottom, to ſhew the outlet of each reſervoir, with their valves.

A. The heart.

B. B. The auricles opened.

C. C. The valves, which are at the outlet of the great reſervoir.

D. The ſemilunar valve, at the outlet of the ſmall reſervoir, which is the third particularity contained in the ſmall land tortoiſe.

E. The partition, which ſeparates the auricles.

Plate IX. *Fig.* 1. repreſents the reſervoirs, the veins which compoſe them, and the arteries: the whole ſeen thro' the upper ſcale, as the animal is in the poſture of walking.

A. The right auricle.

B. The left.

C. The great reſervoir.

D. The *vena cava inferior.*

E. The right axillary.

F. The vein which brings back the blood from the left part of the liver.

G. The left axillary.

H. The place where the great reſervoir is implanted into the right auricle.

II. The two pulmonary veins.

K. Their reſervoir.

L. The place where it is implanted into the left auricle.

M. The principal trunk of the *aorta*, which did not appear in the preceding figures, which repreſent

repreſent the animal reverſed, becauſe it is then hidden by the two other arteries.

The reſt of the diſtribution of theſe veſſels was deſcribed in *Plate* VIII. *Fig.* 2.

Fig. 2. repreſents the great and ſmall reſervoirs.

A. The great reſervoir.

B. Its union with the right auricle.

C. The outlet of the ſmall reſervoir opened.

Fig. 3. repreſents the great reſervoir naked, and ſhews nothing new but

A. The form of its outlet, and

B. The inſertion of the coronary vein.

Fig. 4. repreſents the ſame reſervoir opened, to ſhew the fleſhy fibres, with which it is lined on the inſide.

Fig. 5. repreſents the little reſervoir formed by the concourſe of the pulmonary veins, and its outlet.

Plate X. *Fig.* 1. repreſents the three arteries already deſcribed, cut at the *baſis* of the heart, to ſhew their origin and connection.

A. The principal trunk of the *aorta.*

B. The left branch of the deſcending *aorta.*

C. The pulmonary artery.

Fig. 2. repreſents the heart reverſed on its auricles, to ſhew the diſtribution of the coronary vein.

A. The heart.

B. B. Its auricles.

C. The trunk of the coronary vein.

D. Its inſertion into the great reſervoir.

E. E. Its ramifications.

Fig. 3. repreſents the heart, the three arteries of which are cut at their origin, with its auricles ſwoln; it ſerves principally to ſhew how each auricle, in contracting itſelf, makes a ſort of canal,

Fig: 1.

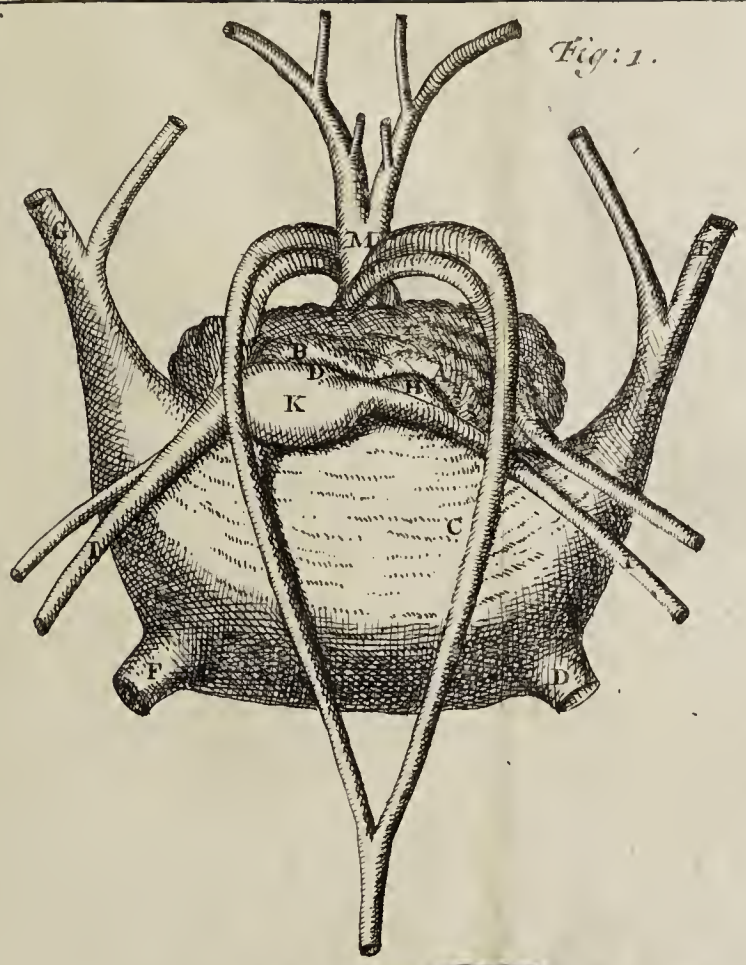
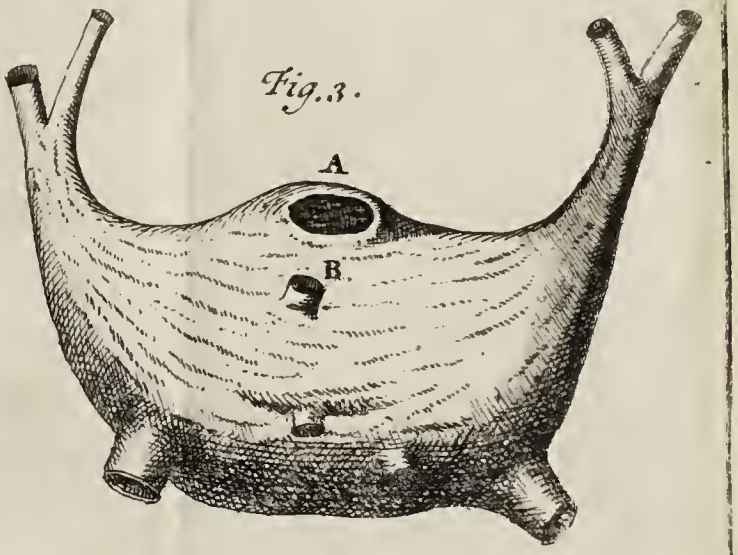


Fig: 3.



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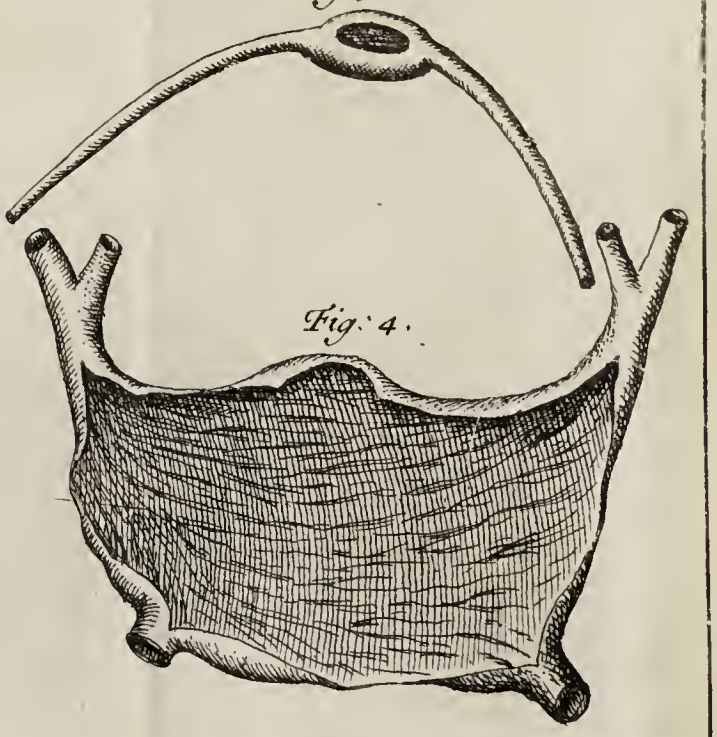

Fig: 2.

Fig: 4.

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canal, which inoſculates with the cavities of the heart. It diſcovers alſo the origin of the coronary artery.

A. The heart.

B. B. B. Its three arteries cut.

C. C. The auricles ſwoln.

D. D. The place where they contract themſelves, and make a canal.

E. The origin of the coronary artery, which comes out of the principal trunk of the deſcending *aorta*, immediately upon the *baſis* of the heart.

Fig. 4. repreſents the heart in the ſame view, the tract of the auricles, and the aperture of their inſertions into the heart with the partition laid down, becauſe being in front it is hardly ſeen.

Plate XI. *Fig.* 1. repreſents the auricles, a part of which has been removed to ſhew their inner texture, the partition that ſeparates them, and the valve at the outlet of the great reſervoir. It repreſents the heart alſo with three pieces removed, one on the right ſide to ſhew the third cavity, and its hole of communication with the firſt; the other on the left ſide, to diſcover the ſecond cavity. Theſe ſections at the ſame time ſhew the different thickneſs of the ſides of theſe cavities. The third piece is removed from the *baſis* of the heart, to diſcover as much as poſſible the ſituation of the valves of the auricles, the fleſhy fibres which compoſe them, and the faſtening of their partition to the middle of theſe valves.

A. The right auricle.

B. B. The two valves in form of eye-lids, at the outlet of the great reſervoir. They are ſhewn more diſtinctly above this figure, at the place marked B B.

C. The partition which ſeparates the auricles.
D. The outlet of the ſmall reſervoir.
E. The heart.
F. The third cavity.
G. Its hole of communication with the firſt.
H. The ſecond cavity.
II. The two valves of the auricles: they are ſeen more eaſily under this figure, marked with the ſame letters.

Fig. 2. repreſents the third cavity of the heart, and the pulmonary artery, half of which has been removed, from the place of its origin to its diviſion.

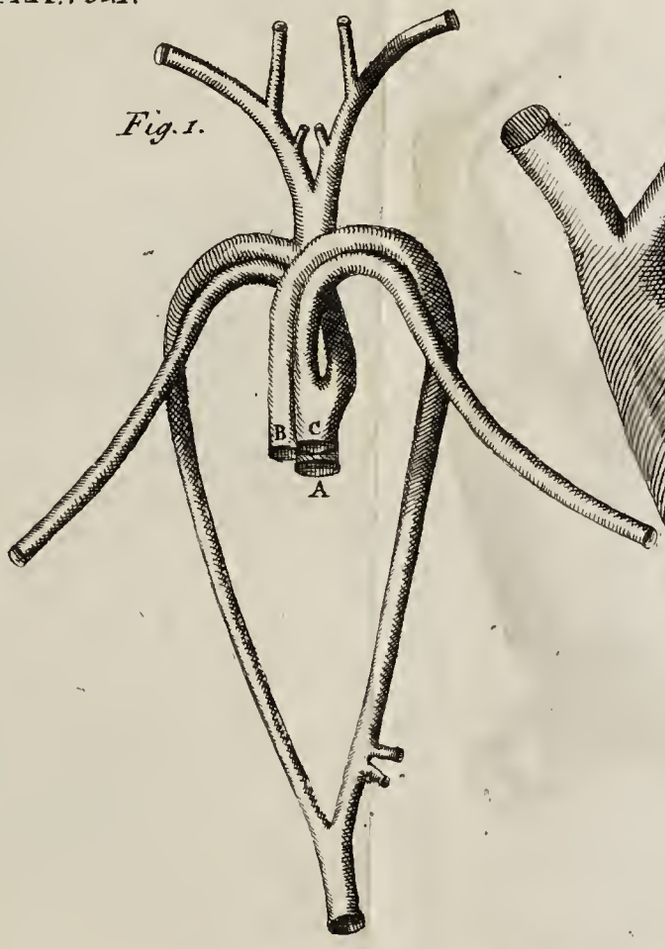
A. The heart.
B. The third cavity opened.
C. The pulmonary artery, which opens immediately into this cavity.
D. One of the two ſigmoidal valves, which are at its outlet. They may be ſeen better in *Fig.* 3 and 4, under the ſame letter. In *Fig.* 3, one of theſe valves is placed on one ſide, and in *Fig.* 4, they are both in their ſituation and blown up.

Fig. 5. repreſents the heart ſeen thro' the upper ſcale, and opened ſo as to ſhew under its *baſis* the two valves at the outlets of the auricles; and a ſmall part of the ſame outlets. The orifices alſo are diſcovered of the two trunks of the *aorta*, and a little on one ſide, and above the hole of communication of the firſt cavity with the third. In the laſt place are ſhewn the firſt and ſecond cavity of the heart in their whole extent, and the paſſage of communication between them.

A. A. A. A. The ſides of the heart which have been ſeparated.
B. B. The two valves of the auricles.

C. C.

Fig. 1.



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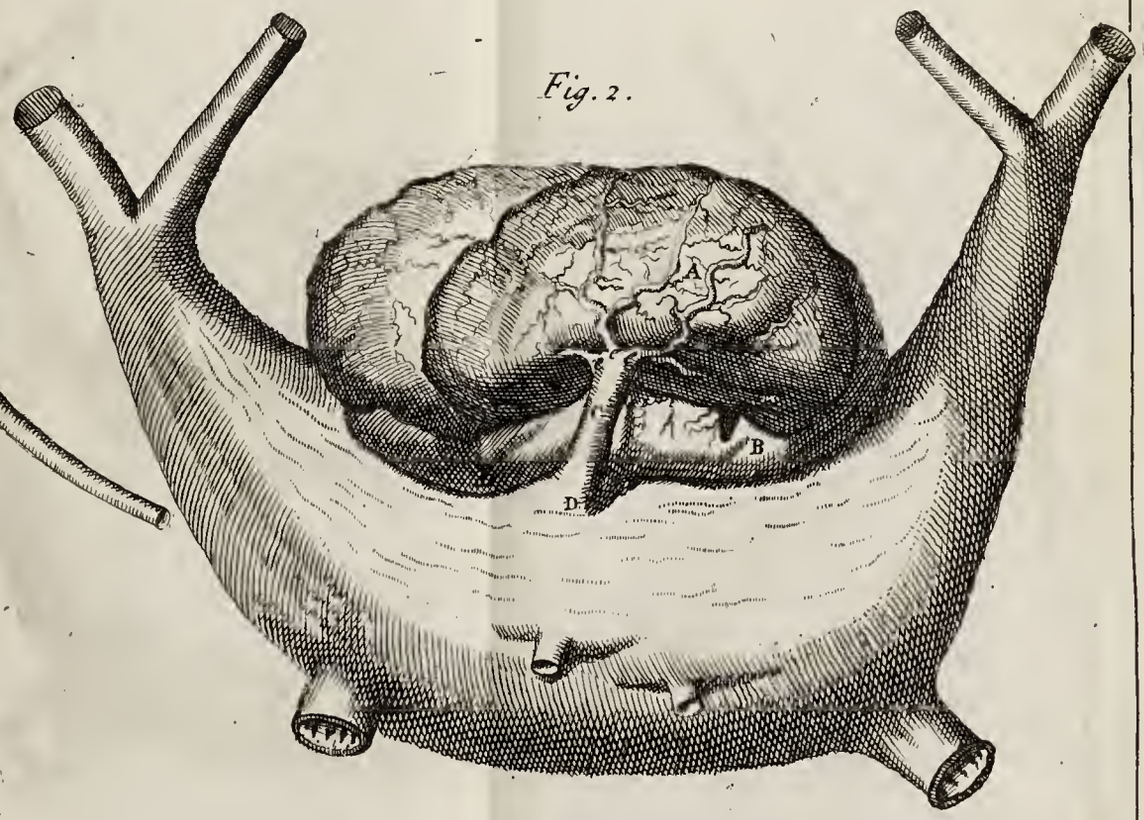

Fig. 3.

Fig. 4.

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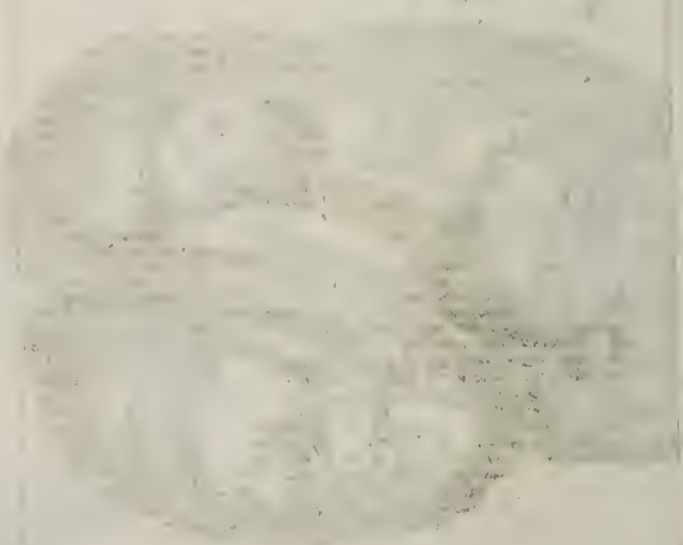


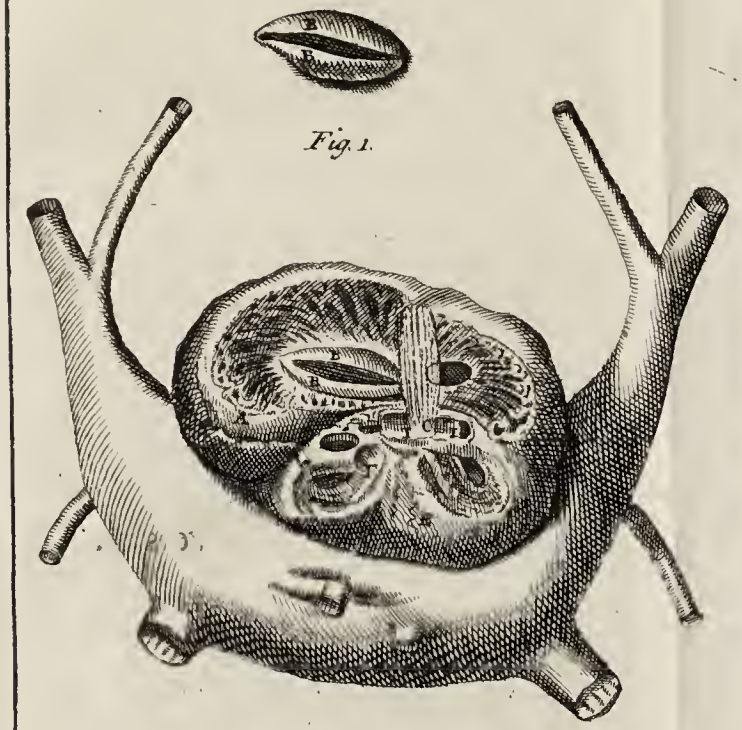
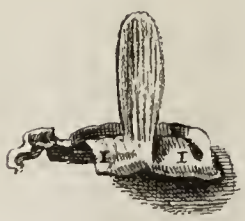


Fig. 1.



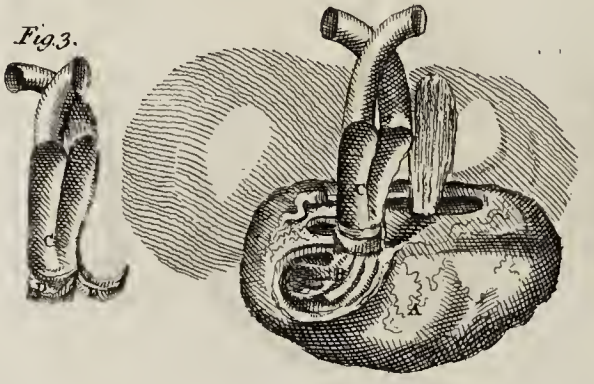


Fig. 2.

Fig. 3.

Fig. 4.

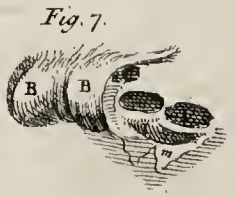


Fig. 7.

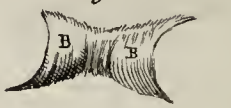


Fig. 6.

Fig. 5.

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C. C. A part of their outlets, which appears under these valves.

D. The orifice of the principal trunk of the *aorta*.

E. That of the left branch of the descending *aorta*.

F. The hole of communication of the first cavity with the third, which is altogether on the right side.

G. The first cavity.

H. The second.

IIII, The pillars of flesh which in rising make a sort of partition, and have been separated from the upper part of the heart.

K. K. The passage of communication.

L. The cartilage, which is fastened to the outlet of the arteries. It has been deprived of a portion of the membrane which covers it, and only one part of it is shewn.

Fig. 6. shews the two valves of the auricles in particular, marked B. B.

Fig. 7. represents the same valves, with the outlets of the auricles, and of the two *aortæ*, and also the cartilage partly divested, and its membrane reversed.

Plate XII. The heart of the frog.

Fig. 1. A. The reservoir.

B. The *vena cava inferior*.

C. C. The veins which return from the liver.

D. D. The axillaries.

E. The coronary vein.

F. The outlet of the reservoir into the auricle.

Fig. 2. represents the outlet of the reservoir into the auricle.

A. The reservoir.

B. Its outlet.

C. C. Two valves in form of eye-lids.

Fig. 3. A. The reſervoir ſeen on the ſide of the ſpine.

B. B. The pulmonary veins.

C. Their trunk.

D. Its outlet into the auricle.

E. The auricle.

Fig. 3. repreſents alſo the ſame parts ſeen on the ſide of the belly, with the auricle, one half of which is cut down from top to bottom.

A. The reſervoir.

B. B. The two pulmonary veins.

C. The outlet of the trunk of the pulmonary vein above the ſuperior valve of the reſervoir.

D. D. The half of the auricle towards the ſpine.

E. E. The two valves of the reſervoir.

Fig. 4. A. The heart opened.

B. B. The two valves, which are at the outlet of the auricle.

C. The auricle opened.

Fig. 5. repreſents the heart, the auricle, its reſervoir, the *aorta*, with its principal branches, and the lungs, of which the right lobe is drawn very faint, to ſhew the veſſels that paſs underneath.

A. The heart.

B. The reſervoir.

C. C. The auricle.

D. The trunk of the *aorta*.

EE. Its two branches, which diſtributing themſelves equally to right and left, are ſubdivided into three others.

F. The ſuperior branch, which divides itſelf into two, of which the exterior makes the carotid.

G. The carotid.

The interior goes to the muſcles which are under the throat.

H. The interior.

I. The branch of the middle, which is the biggeſt. In deſcending it throws off three conſiderable branches, of which the firſt marked K makes the axillary. The ſecond marked L, running under the arm-pit thro' the muſcles of the back divides into two branches, of which the firſt marked M riſes again, and diſtributes itſelf to the muſcles which cover the ſhoulders, and the head. The ſecond marked N deſcending behind the tranſverſe *apophyſes* of the *vertebræ*, throws off ſome branches to right and left; ſome of which go to the muſcles of the back and loins, and the reſt entering by the holes of the *vertebræ*, go to the ſpinal marrow. We muſt thus correct this part in the deſcription, where theſe laſt veſſels are not juſtly deſcribed. The third marked O goes to the œſophagus.

P. The meeting of the two branches of the *aorta*.

Q. The artery which ſupplies the place of the cœliack and meſenterick.

R. R. The third branch of the *aorta*. It divides into two others. The ſmalleſt marked R diſtributes itſelf into the muſcles of the head. The biggeſt marked S is the pulmonary artery, which is divided into ſeveral branches.

Fig. 6. A. The heart.

E. The fleſhy fibres of the *aorta*.

Fig. 6. A. The *aorta* opened.

B. The cartilaginous *lamella* in the middle of the canal.

C. C. The ſigmoidal valves at the origin of the *aorta*.

D. The valve at the extremity of the *lamella*.

E. E. Two other valves, which occupy the reſt of the canal.

Fig. 7. A. The *aorta* opened.

B. B. The circular fleſhy fibres.

C. C. C. C. The four rows of valves, with the tubercles which ſuſtain them. Thoſe of the laſt row are much bigger than the reſt.

Fig. 8. A. The heart of the viper, the veins of which have been taken out, to avoid confuſion.

a. a. The auricles.

B. The deſcending *aorta.*

C. C. The aſcending *aorta.*

D. D. The pulmonary artery.

E. A branch which goes to the ſtomach, and comes from the deſcending *aorta.*

F. The reunion of the two *aortæ.*

G. The left carotid.

H. The right carotid.

I. A branch of the aſcending *aorta*, which goes to the ſpine.

K. K. The branches which go to the lungs, of which the upper one is the biggeſt.

By the ſide of this figure are ſhewn the heart and its auricles diſengaged from all the veſſels.

A. The heart.

B. B. Its two auricles.

Fig. 9. repreſents the heart a little reverſed on the left ſide.

A. The *vena cava ſuperior.*

B. The *inferior.*

C. Their union.

D. The right auricle.

E. The left.

F. The heart.

Fig. 10. repreſents the heart reverſed on the right ſide, to ſhew the left *vena cava ſuperior*, and the pulmonary veins.

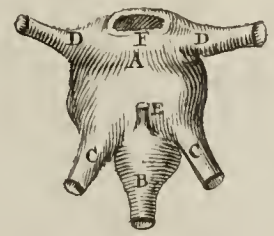
A. The right *vena cava ſuperior.*

B. The *inferior.*

C. The left auricle ſeen ſidewiſe.

D. The left *vena cava ſuperior.*

Fig. 1.



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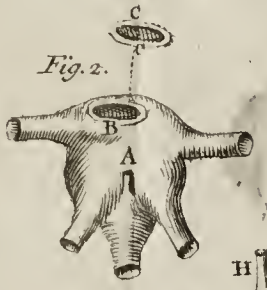

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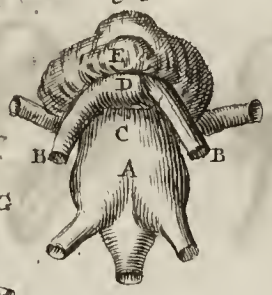

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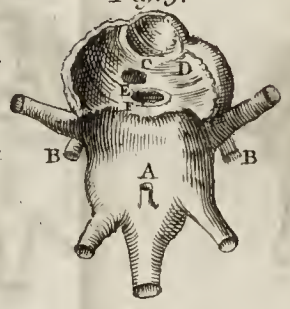

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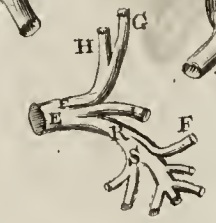

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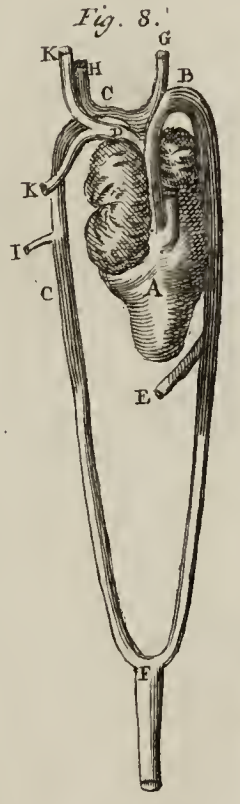

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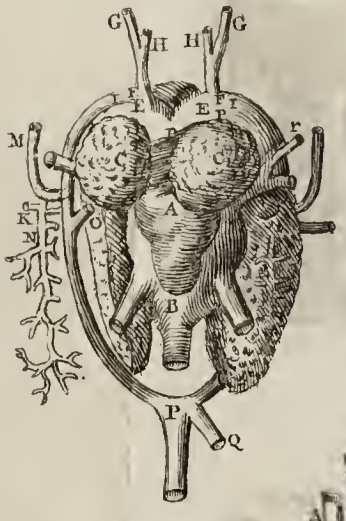

Fig. 6.

Fig. 6.

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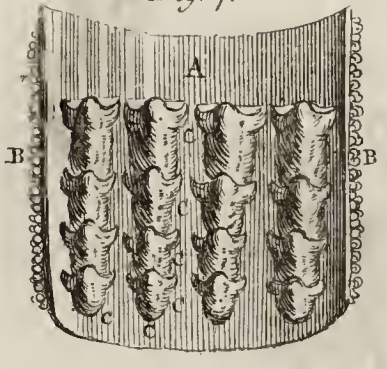

Fig. 9.

Fig. 10.

Fig. 11.

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3. Its outlet into the *vena cava inferior.*

E. The vein of the upper part of the lungs.

F. That of the lower part.

G. The trunk formed by their meeting, and its inſertion into the left auricle.

H. The heart.

Fig. 11. A. The right auricle opened.

B. The meeting of the two *venæ cavæ* on the right ſide.

C. C. The two valves which are at the opening of this vein into the auricle.

Plate XIII. The heart of the carp.

Fig. 1. A. The *pericardium.*

B. The aperture, at which the *aorta* goes out.

Fig. 2. AA. The reſervoir.

B. B. Two veins, which ſupply the place of the *venæ cavæ inferiores.*

C. C. The two *venæ cavæ ſuperiores.*

D. D. D. The three veins, which return from the liver.

E. A vein which brings back a part of the blood from the gills, and neighbouring parts.

F. The auricle.

On the ſide of this figure are the two *venæ cavæ*, and thoſe of the liver, reunited at ſome diſtance from the reſervoir.

Fig. 3. repreſents the auricle cut from top to bottom, to ſhew the outlet of the reſervoir.

A. The reſervoir.

B. The auricle cut.

C. C. The valves in form of eye-lids.

Fig. 4. repreſents the heart reverſed on the right ſide, to ſhew the form of the auricle.

A. The auricle.

B. The heart.

C. The *aorta* dilated.

Fig. 5. A. The heart.

B. B. The two valves which are at the outlet of the auricle.

C. The aperture which is between these valves.

Fig. 6. represents the heart reversed on the left side, to shew its form better, and in what manner the *aorta*, which is very much dilated at its origin, rests upon its base.

A. The heart.

B. The dilatation of the *aorta.*

Fig. 7. represents one of the arches seen above, to shew the gutter, and the two parts which compose it.

A. The first part of the arch.

B. The second part.

C. The gutter.

Fig. 8. represents one of the *lamellæ* in particular, we speak of it here before the vessels, because they are made to support their ramifications.

A. The stalk of the *lamella.*

B. The threads of the convex part. We see that they are tied together by a very fine membrane, but that their extremities are not joined.

C. Those of the concave part.

D. The foot with its gutter.

Fig. 9. represents two *lamellæ* seen in front, and furnished with their threads.

A. The *lamella*, which makes the convex side of the *lamina.*

B. The *lamella*, which makes the concave side of the same *lamina.*

We see by this, that the bony threads are longer in the concave side of the *lamella* A, and shorter in the convex side of the *lamella* B. So that these two *lamellæ* always have their shortest threads towards each other. This is what has not been sufficiently explained in the description, where we have spoken only of the *lamellæ*, which make the convex side of the *lamina.* *Fig.*

Fig. 9. repreſents the *lamellæ* ſeen ſidewiſe, and parted, to ſhew the membrane which connects them, and cord which terminates it.

A. A. A. The membrane, which connects the *lamellæ*.

B. The cord which terminates it. We ſee how this cord forms as many *lunulæ*, as there are ſpaces between the *lamellæ*.

Fig. 10. repreſents the canal formed by the meeting of the gutter with the two feet of the *lamellæ*.

Fig. 11. repreſents the *aorta* opened, to ſhew the fleſhy columns, with which it is furniſhed on the inſide, which makes it very much ſwoln in this place.

Fig. 12. repreſents the diſtribution of the *aorta*.

A. The heart.

B. The auricle.

C. The *aorta* dilated.

D. Its diviſion into four branches on each ſide. We ſee that each of theſe branches running thro' the whole length of the *lamina* terminates entirely in its extremity.

E. E. E. E. Four twigs which detach themſelves from each branch about an inch from their origin, and diſtribute themſelves to the beginning of each *lamina*. The ſame figure ſhews how each branch is divided into as many twigs as there are *lamellæ*.

Fig. 13. repreſents a portion of a *lamina* detached from one of the ſides of the gutter, and a little reverſed, to ſhew how the artery is incloſed in the middle of the void, which the feet of the *lamellæ* leave between them. It is diſengaged from the vein which covers it, and a little drawn downwards,

downwards, for the better discovery of the pairs of branches which it gives to the *lamellæ*.

A. A. The gutter.

B. A portion of the *lamina*.

C. The artery with its branches.

Fig. 14. A. A. represents a pair of *laminæ* deprived of their vessels.

Fig. 14. A. The trunk of the artery inclosed in the gutter of the arch.

B. B. The two branches of the same artery, which rise along the inner edge of each *lamella*.

C. The place where the two branches make an *anastomosis*.

D. D. D. Their little transversal arteries, which cover the flat of each *lamella*.

E. E. The tube which serves for a vein, and is depressed on the exterior edge of each *lamella*, in which also their little transversal arteries immediately inosculate.

F. The trunk of the vein inclosed in the gutter of the arch.

Fig. 15. represents the same arteries detached from the *lamellæ*.

Fig. 16 and 17. represent the distribution of the veins of the gills.

We there see, that the vein inclosed in each arch receives almost $\frac{1}{3}$ of the distance from each of its extremities two branches, each of which returns from each row of the *lamina* to which it is applied, whereas it is the middle of this vein that furnishes itself to the part of the middle of this *lamina*, under which it is couched. This distribution is made thus differently, only to make the course of the vessels, which go to the *lamellæ*, more sure and easy.

A. A. A. The trunk of the vein of the gills, which is couched above the *aorta*.

B. B. B. B.

B. B. B. B. The place where each vein divides into three.

C. C. C. C. The place where theſe veins are inſerted into the trunk marked A.

D. D. D. D. The place where each of theſe veins ſubdivides into three more; of which there are two alſo, which return from the *lamellæ*.

E. E. The place, where theſe veins are reunited by pairs on each ſide.

F. The trunk formed by their meeting. It is to be obſerved, that as ſoon as each of theſe veins comes out of the extremity of the gutter of the arch, the ſides of their canal become thicker, and take the ſame conſiſtence with the arteries, whereas all the reſt of theſe veins is as thin and fine as the fineſt lymphatic veſſel.

G. The branch of the laſt arch, which before the reunion caſts off ſeveral branches, which go to the eye, the noſe, the brain, and to all the neighbouring parts of the head.

It ſupplies the place of the aſcending *aorta*, and the trunk formed by the meeting of theſe veins, which are become arteries, ſerves for a deſcending *aorta*.

Fig. 1.
B
A

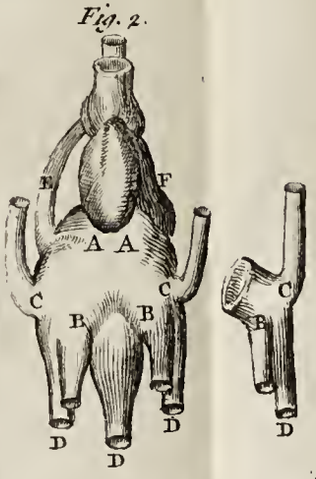
Fig. 2.
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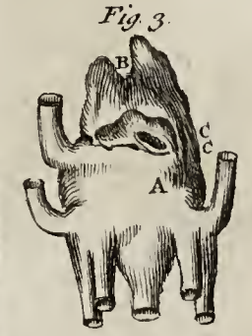
Fig. 3.
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Fig. 4.
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Fig. 7.
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Fig. 8.
B C
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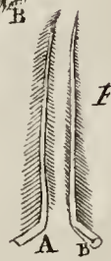
Fig. 9.
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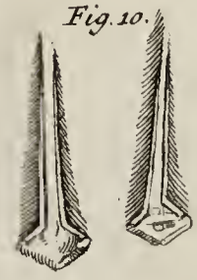
Fig. 10.

Fig. 6.
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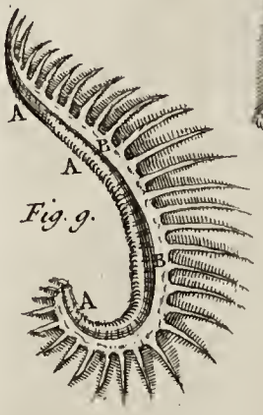
A
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Fig. 9.
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Fig. 11.

Fig. 15.

Fig. 14.
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Fig. 8.

Fig. 14.
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Fig. 13.
C
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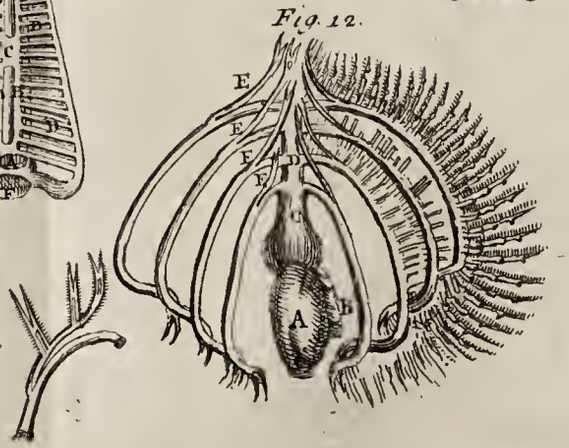
Fig. 12.
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Fig. 15.
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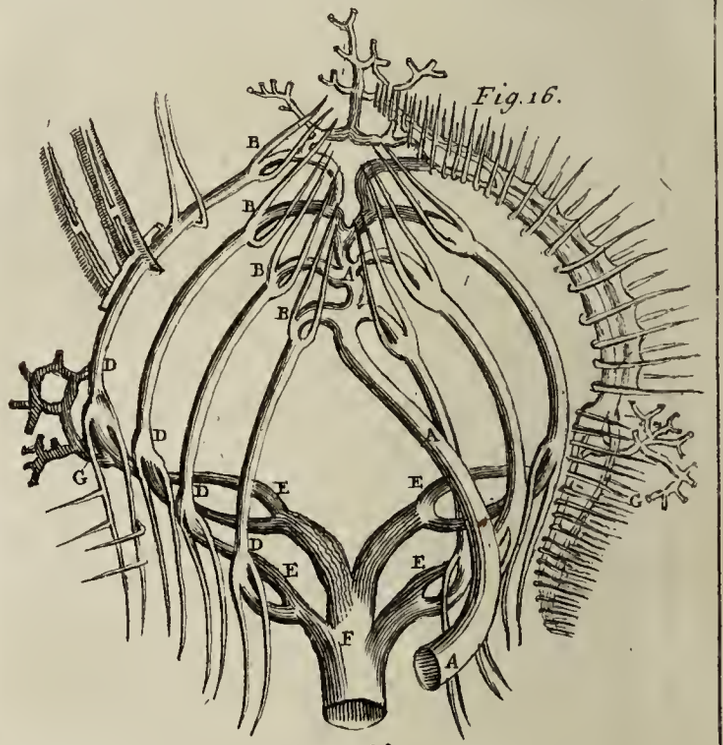
Fig. 16.
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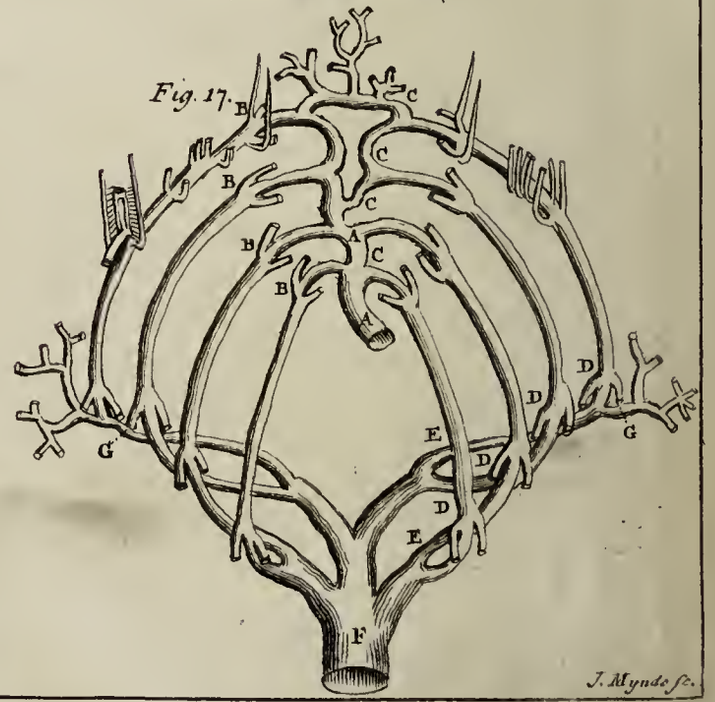
Fig. 17.
B
C
A
D
G
E
F

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A TABLE

OF THE

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II. *On the apparent largeness of the horizontal moon.*
III. *On a body of fire seen in* Normandy.
IV. *On the yellow amber.*
V. *On a change made in the texture of bodies by exterior motion only.*
VI. *Of the trembling of the nerves of a frog after death.*
VII. *Of a stone found in the bladder of a mare.*
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IX. *On a new telescope-glass.*
X. *On the centres of conversion, and on frictions.*
XI. *On the bodies which swim in liquors.*
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II. *A new manner of rendering barometers luminous, by M.* Bernoully, *professor at* Groninguen.

III. *Remarks on the construction of pendulum clocks, by M.* de la Hire.

IV. *An extract of some letters written from* Portugal *and* Brazil, *by M.* Couplet *the son, to the Abbé* Bignon, *president of the Royal Academy of Sciences.*

V. *A general method of throwing bombs, in all cases proposed, with an universal instrument for this purpose, by M.* de la Hire.

AN

AN ABRIDGMENT OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS, in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1700.

I. *On ſome ſingularities of* France.

AS the academy continues its deſign of examining all the wonders of the natural hiſtory of *France*, there has been mention made of the *Montagne de l'Aiguille* in *Dauphiny*, otherwiſe called the inacceſſible mountain. The ſituation of it is reverſe, and it is pitched if I may ſo ſpeak, on its ſummit and point, for it is but 1000 paces round at its baſe, and yet is 2000 at top. Hence it obtains the name inacceſſible: However when *Charles* VIII. went into *Italy* in 1492, he ſent ſome perſons of ſufficient courage and dexterity, to climb up to the top of this mountain. They found only *Chamois* upon it, and it is not eaſy to comprehend, how theſe animals, which had uſed no induſtry, could get thither. There were no trees ſeen upon it, there being only a meadow. The way that they took to go up might be about half a league. Upon the platform of this mountain there is a pointed elevation, on which account it is called *Montagne de l'Aiguille*, or the needle mountain.

Another ſingularity of *Dauphiny* is the grotto of *Notre Dame de la Balme* near *Grenoble*. It opens with a pretty high arch; and leads to a lake

lake incloſed under the mountain, and ſeeming to be a league broad. When *Francis* I. was in *Dauphiny*, he ſent thither ſome people in a boat, who went above 2 leagues upon the lake, but beginning to hear a great noiſe they were afraid, and proceeded no farther, and placed ſome lighted torches upon planks, which they ſaw diſappear in a certain place, which probably was a whirlpool. A clergyman of that country went thither a few years afterwards, and whether he took another way in the grotto, or was leſs eaſily frighted, or had an imagination leſs diſpoſed to the marvellous, he has left a very different, and much more ſimple relation of this voyage. He ſaw ſome falls of water, he found ſome places that were dry, and others where the arch was ſo low, that there was no paſſing without lying down flat in the boat. This laſt circumſtance may give us ſome trouble, but in ſhort the marvellous ſinks conſiderably in the ſecond relation.

This is what has been related in books concerning this grotto, and the inacceſſible mountain. But *M. Dieulamant* has given himſelf the trouble to ſend the academy a relation of the grotto, which he has examined with his own eyes, and it preſerves no farther remains of its ancient wonders. It is irregularly hollowed in the rock, and its entrance may be 4 or 5 toiſes in breadth, and 5 or 8 in height. At the bottom of this entrance, there comes out a little rivulet which flows into the *Rhone*: This rivulet was almoſt dry in *Auguſt*, when *M. Dieulamant* went to ſee the grotto, but he judged by the channel, that it was always very ſmall. The grotto is forked. In the part on the right hand, there are a great many congelations of the water that diſtills thro' the rocks. In the part on the left hand, ſome

waters diſtil, which make part of the rivulet. They fall at firſt into a pretty large natural baſon, below which there are ſeveral other ſmall ones, which make a pretty agreeable caſcade. At the end of this grotto is a ſort of opening hollowed alſo in the rock, at the baſe of which is the water that forms the greateſt part of the rivulet. This is what is called the lake, becauſe the water is dormant. It is half a foot, or at moſt a foot in depth. The alley, where this ſort of lake is, appeared to M. *Dieulamant* to be not above 20 toiſes in length, contracting itſelf a little; for from the beginning where he was, he thought he ſaw the end with torches. The people of the country affirmed that there was nothing beyond. This however is the abyſs where the torches were ſwallowed.

If M. *Dieulamant* had examined the inacceſſible mountain, perhaps that alſo might have been reduced.

The floating iſlands in a lake near St. *Omer*, have alſo been reviewed, and found to be leſs wonderful. They are properly nothing but tufts of the roots of graſs mixt with a little fat earth.

II. *On the apparent largeneſs of the horizontal moon.*

It ſhould ſeem that the apparent largeneſs of an object, muſt depend on the largeneſs of the image that it traces on the bottom of the eye, and yet ſometimes the contrary happens, and the moon of which our eye receives a ſmaller object at the horizon than at the meridian, becauſe it is then farther from us, appears much larger at the horizon.

This

This *Phænomenon* has very much embaraſſed the greateſt philoſophers among the moderns, as it often happens that when we give very different explications of one and the ſame thing, none of them is true. F. *Gouye* has not been contented with what has hitherto been imagined on this ſubject: *Deſcartes* ſays that when the moon riſes or ſets, a long ſeries of objects oppoſed between us, and the extremity of the ſenſible horizon, make us imagine it farther off than when it is in the meridian, where our eye ſees nothing between it and ourſelves, that this appearance of a greater diſtance makes us imagine the moon larger, becauſe an object that we ſee under a certain angle, and think at the ſame time to be very far off, naturally makes us judge it to be very large, to appear ſo far off under that angle, and thus a mere, tho' neceſſary judgment of our mind, which is common to all men, makes us ſee the moon larger at the horizon, in ſpite of the ſmaller image that is painted on the bottom of the eye. But F. *Gouye* at once deſtroys this ſo ingenious an explication, by affirming that the more the horizon is bounded, the more large the moon appears to us. *M. Gaſſendi* pretends that the pupil of the eye, which is conſtantly moſt open in the dark, being more ſo in the morning and evening, becauſe thicker vapours are commonly ſpread over the earth, and becauſe it muſt alſo traverſe a longer way to look at the horizon, the image of the moon enters the eye under a greater angle, and paints itſelf really larger. But notwithſtanding this dilatation of the pupil cauſed by the darkneſs, if we look at the moon thro' a little tube of paper, we ſhall ſee it ſmaller at the horizon.

To

To find any reaſon therefore for ſo odd a *Phænomenon*, *F. Gouye* conjectures that when the moon is in the horizon near the earth, the thicker vapours, with which this planet is then encompaſſed with regard to us, have the ſame effect with a wall placed behind a column, which then appears much bigger than if it ſtood by itſelf, and was encompaſſed on all ſides by an enlightened air. Beſides a fluted column appears bigger than a plain one, becauſe the flutings make ſo many particular objects, which by their multitude give room to imagine, that the total object which they compoſe is of a greater bulk. It is much the ſame with all the objects that are ſpread on that part of the horizon, with which the moon correſponds when it is near them; which cauſes it to appear greater when it riſes behind trees, the intervals between which being cloſer and more obſervable, have almoſt the ſame effect on the apparent diameter of this planet, as a greater number of flutings would have on the body of a pillar.

III. *On a body of fire ſeen in* Normandy.

The 7th of *Jan.* an hour before day, there appeared to the inhabitants of *la Hague* in *lower Normandy*, a body of fire ſo bright that it eclipſed the light of the moon, and that the inhabitants of *S. Germain des Vaux*, and of *Auderville*, two great villages ſituated on the banks of the ſea, thought it at firſt to have been day, and were much frightened at ſuch a prodigious light. The figure of this fire was like a great tree, and ran from W. N. W. to E. S. E. it did not fall till an hour after day-light, and then with ſo great a noiſe, that the houſes of thoſe two villages ſhook

with it. They who were 12 leagues from *Cherbourg*, thought it had fallen on *Valognes*, and they of *Valognes* thought it had been on *Cherbourg*. But as the inhabitants of *la Hague* were the only ones who heard the noiſe, and felt the ſhock cauſed by its fall, they are the more credible witneſſes in this point. It appear'd to them as if this flame was loſt in the ſea, near the little iſle of *Origni*, and this appearance was not much unlike that of a great ſhip on fire. The academy was obliged to M. *de Seneſſey* a gentleman of *lower Normandy* for the information they received of this *phænomenon*.

IV. *On the yellow amber.*

It is commonly thought that yellow amber, which is found in the *Dantzick* ſea, is a gum which is produced, and falls from certain trees ſituated on the banks of this ſea. But M. *Tournefort* received an account from *Aix*, that yellow amber is to be found in the moſt bare and barren cliffs of the rocks of *Provence*; which ſhould incline us to believe that this gum is mineral and not vegetable; and that the amber which is in the ſea of *Dantzick*, did not fall therein from trees, but was conveyed thither by land floods.

V. *On a change made in the texture of bodies by exterior motion only.*

It is well known what force an inward motion hath, when diffuſed thro' all the inſenſible parts of a body to change the texture of it, and produce great alterations. But that a meer outward motion, which ſeems to terminate in the whole maſs without agitating the parts, ſhould produce the ſame effects, is ſomething more ſurprizing,

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at leaſt to a natural philoſopher. M. *Homberg* ſays, that having tied a bottle of wine to the clapper of a mill, he found that the motion of this clapper alone had changed the wine into very good vinegar in the ſpace of three days; and that a pound of quickſilver had by the ſame means in three months time, given 4 or 5 ounces of a blackiſh powder.

VI. *Of the trembling of the nerves of a frog after death.*

M. *Du Verney* ſhewed a frog juſt dead, which in taking the nerves of the belly of this animal which go to the thighs and legs, and irritating them a little with a ſcalpel, trembled and ſuffered a ſort of convulſion. Afterwards he cut theſe nerves in the belly, and holding them a little ſtretched with his hand, he made them do ſo again by the ſame motion of the ſcalpel. If the frog had been longer dead, this would not have happened, in all probability there yet remained ſome liquor in theſe nerves, the undulation of which cauſed the trembling of the parts where they correſponded, and conſequently the nerves are only pipes, the effect whereof depends upon the liquor which they contain.

VII. *Of a ſtone found in the bladder of a mare.*

The academy has ſeen in the hands of M. *Lemery*, a ſtone found in the bladder of a mare, which weighed 23 ounces 7 drams. It is 4 inches $\frac{1}{3}$ in diameter one way, and 4 $\frac{1}{2}$ the other. It is the ſize of a midling melon, covered with a ſort of ſmooth and ſhining ſkin. Its ſubſtance

is aſh-colour, and it is hardened ſince it has been drawn from the animal, altho' it is ſtill very friable. It has alſo loſt in drying a very ſtrong ſmell of urine, which it had at firſt.

VIII. *Of a monſtrous double leveret.*

M. *Lemery* brought alſo to the aſſembly a monſtrous leveret, or rather two leverets joined together from the head to the breaſt. They had but one head and one face, tho' they had four ears. In the place for the mouth there was only a little cavity without any opening to receive the nouriſhment. Nevertheleſs they lived, and even out of their mother's belly; for they were taken with the hand by a huntſman. This double animal was walking in a wood, but one of the leverets drew one way, and the other another way, ſo that they did not get forward at all. They told M. *Lemery*, that in opening them, they had found in each a heart, lungs, and ſtomach, all ſound.

IX. *On a new teleſcope-glaſs.*

M. *Tſchirnhaus* has informed the learned of the effects of a new glaſs which he has made.

This glaſs is convex on both ſides, and 32 feet of *focus*, but it is extraordinary by the largeneſs of its diameter; for as the greateſt glaſſes of the ſame *focus*, which have hitherto been uſed are only 4 or 5 inches in diameter, this is more than a *Rheniſh* foot, and at firſt it had two feet, but it was injur'd by accident. From hence we may judge what the machine muſt be that M. *Tſchirnhaus* has invented to cut ſuch great glaſſes.

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All dioptricks ſeem to be reverſed by the effects which it produces. For example, the common object glaſſes of teleſcopes, tho' ſmall enough already, have very little aperture left them, and yet M. *Tſchirnhaus* leaves his quite open, notwithſtanding it is ſo large.

To underſtand the reaſon of the common practice, and the particularity of this of M. *Tſchirnhaus*, you muſt know exactly how the image of a luminous point, the rays of which have traverſed it, is made in the *focus* of a convex glaſs. Let us ſuppoſe this glaſs to be an entire hemiſphere. The rays that the luminous point ſends upon the ſurface of this hemiſphere, do not reunite themſelves, after having croſſed it, upon one ſingle point of its *axis*, as we might imagine, from the rules of the *focus*, and of the reunion commonly uſed in this ſubject. On the contrary they have a pretty large ſpace, but much leſs than the ſurface of the hemiſphere on which they were before ſpread. Thus altho' they are not exactly reunited, they are more cloſe one to the other than they were. Moreover, in the extent of the ſpace which they fill, they are unequally cloſe, leſs ſo toward the extremities, and much more toward the middle, that is toward the *axis* of the hemiſphere, and there one would think they were entirely united in one point. Theſe rays nearly united upon the *axis*, are thoſe which are entered toward the *axis*, that is, towards the middle of the hemiſphere, and as all the others, which fall upon the reſt of the ſurface, unite very ill, they take away all this remainder of the hemiſphere, and preſerve only the middle of it to make the objects. But there is alſo another obſervation to be made. The rays which have been refracted,

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naturally form colours and *Iris's*, unleſs they are not reunited by the refraction, or at leaſt extremely locked againſt each other. Thus an object glaſs, which is only a little remnant of the hemiſphere, is nevertheleſs commonly very large, becauſe it receives rays towards its edges, which after being refracted do not again cloſe themſelves enough, and make *Iris's* very inconvenient to the obſervers. It is for this reaſon they cover the edge of the object glaſs, and that they leave only a little round extent about its *axis*.

M. *Tſchirnhaus* diſapproves of this practice, in all appearance ſo well founded, and ſuppoſes there are many other errors in opticks, which ſhould be rooted out. He does not leſſen at all the great ſurface of his object glaſs, but he has ſome ſecret about it, which he has not yet diſcovered. It is always certain, that it would be advantageous to leave a larger aperture to the object glaſſes provided there is no other inconveniencies, becauſe there would come more rays to the ſame point.

M. *Tſchirnhaus*'s object glaſs may be uſed without an eye-glaſs, which is a very great advantage, for the more glaſſes there are in a teleſcope, the more rays there are reflected upon their ſurfaces which are loſt to the obſervers. M. *Tſchirnhaus* alſo aſſures us, that the objects ſhew themſelves more clear with his ſingle glaſs, than they have yet been ſeen with teleſcopes. He affirms alſo, that they appear more clear, than to the naked eye, altho' ſome reflections of rays are neceſſarily made upon this glaſs, but this loſs is probably more than recompenſed by the great number of rays in the ſame point, which the great extent of its ſurface and *focus* cauſes to enter into the eye.

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This glaſs may be uſed without a tube, and in another manner than that which M. *Huygens* mentions in his aſtroſcopy. For the object ſhews itſelf always diſtinctly, notwithſtanding the rays of the ſun which paſs between it and the eye.

The ſpace which may be ſeen at once with this glaſs, is of an incredible ſize. M. *Tſchirnhaus* affirms, that without tube or eye-glaſs, he has ſeen very diſtinctly at noon, an entire town at the diſtance of a *German* mile and half.

So many ſingularities of M. *Tſchirnhaus*'s glaſs foretel great and happy novelties in dioptricks.

X. *On the centres of converſion, and on frictions.*

If a ſtick be laid upon a ſtagnant water, and drawn by a thread fixed to it, ſo that the thread makes always the ſame angle with the ſtick, for example, always a right angle, the ſtick will be found to turn upon one of its points which will be immoveable, which M. *Parent* calls the centre of converſion. We may ſuppoſe for the greater eaſe, that the thread be faſtened to one end of the ſtick.

This effect ariſes from the reſiſtance of the fluid, and from the manner wherein it divides. For let us imagine the firſt moment of the traction; it is certain, that the reſiſtance of the parts of the fluid which are to be diſplaced, tends to make the ſtick turn about the point where the thread is faſtened, as about a centre, ſo that in the preſent ſuppoſition, the ſtick deſcribes exactly a quadrant of a circle, after which the fluid would no longer reſiſt the ſtick lengthwiſe. But if this circular motion is executed, the free end of the ſtick, and

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the parts which are neareſt to it, would deſcribe longer arches of circles than the others, and have a greater velocity. The reſiſtance therefore of the fluid, which tends to impreſs a circular motion on the ſtick about the point where the thread is tied, tends to impreſs a greater velocity on the parts, which are towards the other extremity, or, which comes to the ſame thing, theſe parts have need of a greater velocity, to ſurmount the reſiſtance of the fluid, ſo that the ſtick does not take this circular motion about the point where the thread is, or the reſiſtance of the fluid is greater towards the free end of the ſtick, and ſtill leſſens from thence towards the other end. Now we muſt ſuppoſe, that all the columns or threads of water, which reſiſt the ſtick, are of the ſame length, or of the ſame maſs. Therefore we may find upon the ſtick ſuch a point, that taking a greater number of theſe threads on the ſide where they reſiſt leaſt, and a leſs number on the ſide where they reſiſt moſt, there will be an exact compenſation, and the forces will be equal on both ſides. It is this point which is the centre of converſion, and as the ſame reaſoning has place in all moments of the traction, which is made always in the ſame manner, this centre is always the ſame point.

If the angle of the thread with the ſtick ſhould ceaſe to be a right one, and become obtuſe, the centre of converſion would nevertheleſs be always at the ſame point, becauſe the reſiſtance of the fluid, altho leſs in itſelf, is always according to the ſame proportion, greater towards the free end of the ſtick, and leſs towards the other end, which appears viſibly by the circular motion which it always tends to impreſs on the ſtick, altho' this motion need not be an entire quadrant of a circle. But if, after having drawn the ſtick by its

its thread under a right angle, we draw it under an obtuſe one, the centre of converſion, which would not change its place upon the ſtick, would change it in the fluid, becauſe the whole ſtick would make a motion, and we ſhould not ſee it turn any longer upon the ſame immoveable point.

The grand queſtion is to know preciſely at what point the centre of converſion is to be found, and it is this, which M. *Parent* has determined by an algebraical calculation, adding ſome other general conſiderations of mechanicks, beſides thoſe which are particular to this *phœnomenon*, and which are the only ones that we have related; he finds that if the ſtick drawn by one extremity be a ſingle line, divided into 20 parts counting from the thread, the centre of converſion will be nearly on the 13th. If it be not any more a line, but a ſurface, or a ſolid that is drawn, there will be ſome change in the ſituation of the centre of converſion, according to the ſurface or the ſolid. All this requires a great detail of algebraical calculations, into which M. *Parent* has entered.

If inſtead of ſuppoſing here that the body drawn ſwims upon a fluid, we ſuppoſe it upon a rough uneven plane, the reſiſtance of the plane to the motion of the body will always divide itſelf in the ſame manner, and determine the ſame centre of converſion.

This reſiſtance is exactly what they call the friction, ſo prejudicial to the effect of all machines; and the theory of centres of converſion which might appear at firſt very little uſeful, has been of ſervice to M. *Parent*, in the inquiries and determination of the force of frictions.

To confine in certain bounds this queſtion, which makes innumerable and conſequently in-

ſolvable, the endleſs differences of ſurfaces which may rub together, M. *Parent* conſiders only one body perfectly hard, which rubs either upon a very hard body, or a very ſoft one, that is, a fluid. Theſe two extremes being once known, we ſhall judge pretty nearly of all thoſe which are between them.

Each of theſe two caſes divides again in two, for it is a queſtion to know the neceſſary effort either to move the leaſt that is poſſible, that is to begin to move one body upon another, or to move them with a certain given quickneſs.

If the body, upon which the very hard body muſt move, is fluid, experiment alone can determine the ſize of the firſt effort, but this firſt effort being once found, thoſe which will be neceſſary to impreſs ſuch velocity as we would, will be between themſelves as the ſquares of theſe velocities, for the reſiſtance of the fluid does not increaſe only, becauſe the body, which is moved upon it, is moved faſter, but alſo becauſe being moved faſter, it meets and ſhocks many more particles of the fluid, which makes a double *ratio*, or ſquares.

If the body upon which the very hard body is moved, be alſo very hard, M. *Parent* conſiders the two ſurfaces of them as quite ſet round with equal hemiſpheres, ſo that the hemiſpheres of the upper bodies are engaged in thoſe of the lower. They muſt be diſengaged and raiſed to begin the motion. This figure helps M. *Parent* to make a calculation, and he finds by pure geometry that in this ſuppoſition, to raiſe a weight of 20 *lb.* there would be required a force which was either 14 *lb.* when one of the hemiſpheres of the upper body muſt be raiſed above two of the lower, or, only 7 *lb.* when it was to be raiſed above one.

Now as these two cases meet at once with regard to the different parts of the same surface, there is then required a mean force between 14 and 7, or between 11 and 10. This proportion is exactly that which M. *Amontons* has determined by the experiment of glasses and their polishers, which are very hard bodies, and which wear very little by one single friction, which brings them very near the supposition of M. *Parent*.

When the lower body is covered with any fat substance, the intervals of the hemispheres of its surface are filled in such a manner, that those of the upper body do not engage themselves any more, and may begin to move themselves without being sensibly raised. Then in this case, the force, which moves the weight of 20 *lb*. must be less than the smallest which can move in the case where it should raise the hemispheres to their whole height; that is, this force must be less than 7 *lb*. M. *Amontons* has found by the experiments related in the memoirs of 1699 *, that it is the third part of the weight, and consequently in the present supposition 6 *lb*. $\frac{2}{3}$ which is under 7. M. *Parent*'s geometry, altho' in a matter which seems to have escaped geometry, has conducted him to the same point where M. *Amontons* is arrived by experiment.

The comparison of different velocities with which we would move one of these bodies upon the other, always according to the hypothesis of the hemispheres, has thrown M. *Parent* into a yet more complicated and delicate geometry. All that we can here gather which is clear and and sensible enough, is, that the resistance of the lower bodies to the motion of the upper does not increase in the same *ratio* with the velocity of the upper

* Pag. 69. of this abridgment.

one, and even does not increaſe more ſenſibly beyond a certain point, altho' this velocity increaſes alſo as much as you will.

Let us imagine with M. *Parent*, the upper body entirely reduced to only one of the hemiſpheres of its ſurface, without loſing any of its weight, and that this hemiſphere paſſes ſucceſſively, and with a horizontal motion, upon a row of lower hemiſpheres, equal to it, falling at each moment between their intervals, and raiſing it ſelf continually. If the weight of this upper hemiſphere be ſuch, that it muſt in a ſecond make it run over a vertical ſpace equal to the height or depth of the intervals, which are between the lower hemiſpheres, and if the horizontal motion of the upper hemiſphere is ſuch, that it makes in a ſecond but one interval of two lower hemiſpheres, it is certain that at each ſecond it will ſink entirely into one of theſe intervals, that is, it will inſert it ſelf as much as poſſible. But if by its horizontal motion, it can run over two intervals in one ſecond, it will enter into each but half of its depth, becauſe theſe two halves put together will make all the height, which its weight ought to cauſe to fall in one ſecond. It is then viſible that the more the horizontal motion, (which may increaſe without end) is great with reſpect to the vertical motion of the weights, which cannot change, ſo much the leſs will the upper hemiſphere ſink and inſert itſelf into the lower ones, and in fine the horizontal velocity may be ſuch, that the upper hemiſphere will not touch the lower one but by their ſummits, after which the friction will not encreaſe more ſenſibly by the increaſe of the velocity. But it muſt be well obſerved, that the friction will never be null, becauſe let the horizontal velocity be ever ſo great,

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as it cannot but be finite and determined, that of the vertical motion of the weight is always ſomething with regard to it.

The more quick a body moves in a fluid, the more parts it meets, the multitude of which is a greater obſtacle; and on the contrary the more quick a body moves upon a very hard body, the leſs it is engaged in the intervals of its parts, and conſequently is leſs detained by them.

XI. *On the bodies which ſwim in liquors.*

It is well known, that a body of the ſame ſpecific gravity with a liquid in which it is immerged, remains in whatſoever place it is put; but if it is lighter than the liquid, it ſwims upon it, having one part immerged more or leſs, according to the proportion of its gravity, to that of the liquid, and if it is heavier, it goes to the bottom. All mathematicians have demonſtrated the neceſſity of theſe effects.

This is only with regard to the place of a body in a liquid, but the ſituation of this body in the ſame liquid, is different from the place which it there occupies. For example, a ſphere which there makes a half turn upon its centre, would change its ſituation, becauſe the upper part would become the lower, but it would not change its place.

Let us imagine a body compoſed of two different parts immerged in water, one lighter, and the other heavier than the water, as of wood and lead, ſuppoſe alſo the quantity of theſe two parts ſo proportioned, that the whole together weighs exactly as much as an equal bulk of water. Imagine laſtly the body to be ſpherical. It is plain that more than $\frac{1}{2}$ the ſphere will be wood.

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This body will have two centres; one, its centre of figure and extent, that is, the point of the middle of the ſphere: the other, its centre of gravity, that is, the point of a horizontal diameter by which it ſhould be ſuſpended, that the two parts of wood and lead, of unequal weight, may remain in *equilibrio*. Now we know that to put them *in equilibrio* the point of ſupport, or which is the ſame thing, the centre of gravity, muſt be neareſt to the heavieſt, and conſequently the centre of gravity of this ſphere, heterogeneous in its parts would be more toward the ſide of the lead, and would not be the ſame point with the centre of the figure.

This ſphere being immerged in water, ſo that its centre of gravity be above it, it is aſked what will happen to it. *

It is certain that a body which has liberty to move itſelf, cannot reſt till its centre of gravity is ſunk as low as it is poſſible. The centre of gravity of the ſphere, that is, the ſphere itſelf muſt then ſink, ſince it can.

On the other ſide, ſince it is of the ſame ſpecific weight with the water, it ought neither to riſe nor fall, but remain exactly in the ſame place where it was laid.

To reconcile theſe two things, M. *Borelli* in his excellent treatiſe of the motion of animals,* determines, that the ſphere will turn upon its centre of figure, until its centre of gravity be ſunk to the lower part, and toward the earth, provided the body changes the ſituation, and not the place.

This deciſion of ſo great an author, and in appearance ſo convincing, has not however ſeemed ſo to M. *Parent*; and he has not been

* Part I. Cap. 23. Prop. 207.

afraid

afraid to advance this paradox; that the body, notwithſtanding the equality of its ſpecific weight with that of the water, would deſcend to a certain point, and what will appear yet more ſurprizing, that the figure of the body would riſe, while the body it ſelf deſcended.

That two equal forces, acting by lines parallel to each other, draw a rod by one end two oppoſite ways, is certain, ſince we do not ſuppoſe any fixed point of this rod, that it will turn upon one of its points, and that this point will be the middle, becauſe theſe two oppoſite forces are equal. If they were not, it would turn upon another point, as much nearer the greater force, as this greater force ſhould exceed the other.

The ſphere immerged in water is driven downward by its own weight, of which all the action is united again in the centre of gravity. It is at the ſame time driven upward, by the weight of a bulk of water equal to it, which always endeavours to deſcend, and take its place, and all the action of this equal body of water, muſt be conceived as reunited in the centre of figure of the ſphere; for we are conſidering the extent of the bulk, and it is this which determines the quantity of water of which we are conſidering the action. Here are two oppoſite forces which act parallel, as theſe are two weights, they are equal by the ſuppoſition, they are applied at two different points taken on the ſame diameter of the ſphere. The ſphere therefore muſt turn upon a middle point, between theſe two, and it muſt be equally diſtant from them.

As the centre of gravity of the ſphere had been placed above, it cannot turn upon this middle point, without the centre of gravity deſcending, and at laſt when it has deſcended to the

the vertical line, its action ceaſes becauſe it cannot deſcend any more, and the ſphere ſtops.

On the other ſide, it is plain, that in this diſpoſition the centre of the figure which was below the centre of gravity, roſe, and from thence M. *Parent* concludes, that the body, which is conſidered as entirely gathered together in its centre of gravity, deſcended, while the figure of the ſame body roſe.

The ſpherical figure was only taken for an example, and it is clear, that the ſame reaſoning ſubſiſts with all other figures.

It ſubſiſts again when the two forces are unequal.

If the weight of the heterogeneous body immerged in water, is greater than that of an equal bulk of water, and its centre of gravity has been placed above, not only this body muſt immerge itſelf in the liquid, but it muſt make a half turn in immerging, becauſe its centre of gravity muſt deſcend as low as is poſſible; after which the body continues to immerge, but without turning any more. It turns round upon a point which is not equally diſtant from the centres of gravity and of figure, becauſe the two forces which are applied to it are unequal.

Hence it comes, that cats, and many other animals of the ſame kind, as martins, pole-cats, foxes, tigers, *&c.* when they fall from a high place, generally fall upon their feet, altho' at firſt they were uppermoſt, and conſequently they ſhould have falled upon their heads. It is very certain, they could not turn themſelves thus of their own accord in the air, where they have no fixed point to help themſelves. But the fear with which they are ſeized makes them bend the ſpine of their back, ſo that their entrails are driven upwards

upwards at the ſame time they ſtretch out the head and legs toward the place from whence they are fallen, as if to find it again, which gives to theſe parts a greater action to raiſe themſelves. Thus their centre of gravity comes to be different from the centre of figure, and placed above it, from whence it follows by the demonſtration of M. *Parent*, that theſe animals muſt make a half turn in the air, and turn their legs again downwards, which commonly ſaves their lives. The niceſt knowledge of mechanicks would not do better on this occaſion, than this confuſed and blind ſenſe of fear does.

If the weight of the body is leſs than that of the water, only one part will be immerged, and we muſt conſider only the part immerged as a centre of figure, becauſe it is that alone which tries and receives the action of the water, one of the two unequal and oppoſite forces. As for the centre of gravity, it is always that of the entire body, becauſe it is always the whole weight that acts. After this, it is eaſy to judge by what has been ſaid how it turns round.

While the body makes its half turn, if we ſuppoſe the centre of turning fixed, as it is naturally and by itſelf, the part immerged in the water will be always equal, if this body be a ſphere; but if it be of any other figure, the action of turning will cauſe this part immerged to be ſometimes greater and ſometimes leſs. On the other ſide, the *equilibrium* of the body with a certain determinate bulk of water requires, that its part immerged ſhould always fill the place of it, which is always of the ſame ſize. Thus the action of turning round appears contrary to what the *equilibrium* would, but what ſettles all is, that when by the turning round the part immerged is too little,

this *equilibrium* makes the body ſink more, and conſequently its centre of turning deſcends in a vertical line. If the part immerged be too great, the body raiſes itſelf, and alſo its centre of turning.

It is manifeſt, that in both theſe motions, the centre of the whole figure riſes or falls in a right line, and at the ſame time turns circularly, and conſequently deſcribes a cycloid, for it is formed by a direct and circular motion mixed in this manner.

To theſe proofs, which ſhew plainly enough that in all theſe caſes the bodies turn, not upon their centre of figure, but on a mean point between this centre and that of gravity, M. *Parent* adds another, as convincing, but falling into a greater geometrical diſcuſſion. The principle is, that the common centre of gravity, both of the liquid and of the body immerged, muſt always be as low as is poſſible, after which M. *Parent* demonſtrates, that it has this ſituation only in his ſyſtem.

He finds by the ſame ways, what ſituation heterogeneous bodies immerged at the ſame time in ſeveral liquors of different weights, muſt take. The principals do not change at all, but the application of them becomes more difficult.

The different figures which we may imagine of this body, throw alſo this problem into a greater detail of geometry; *Archimedes* has reſolved it for the right paraboloid placed only in a liquid. But M. *Parent* reſolves it for all the conoids ſituated in ever ſo many liquors.

XII.

XII. *A falſe report of the perpetual motion being diſcovered, and the impoſſibility of it demonſtrated.*

There was in this year a report ſpread, that the perpetual motion was found. It was ſeen in a place where the difficulty of the thing was not well known, where the invention was not examined, as it would have been in an academy, where an air of ſcience ſucceeds ſometimes, and the air of confidence almoſt always. M. *Sauveur* explained the invention to the academy, who were very much ſurprized at it. A little while after the noiſe that this diſcovery made, the perpetual motion diſappeared with its author. On this occaſion, M. *Parent* proved the impoſſibility of it by this ſingle reaſon, that all the parts of a machine have a common centre of gravity, that while they turn round an axis or fixed point, which ever it be, this common centre of gravity finds itſelf neceſſarily in one ſituation, where it is lower than in any other, and that preſently all muſt ſtop. For ſince there is a point where the force, which many bodies have to deſcend, is entirely reunited, as ſoon as this point cannot deſcend any more, all theſe bodies muſt remain fixed. M. *Parent* determined in general, that there muſt inevitably be this point of reſt for all the machines poſſible.

AN ABRIDGMENT OF THE PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris* for the year 1700.

I. *An obſervation on the barometer, thermometer, and quantity of rain and ſnow-water, that fell at* Paris *in the royal obſervatory, during the year* 1699. *By M.* de la Hire *.

THE obſervations on the quantity of rain water which fell in the obſervatory, have been made in the ſame manner with thoſe of the preceding years. For this purpoſe, they placed in the open tower a veſſel of tin, whoſe ſurface was 4 feet, and which was ſurrounded with a rim 6 inches high. This veſſel is a little bent near one of its angles, where there is a little hole with an end of a pipe, which conveys all the water which falls in the veſſel, into an earthen pot placed underneath; and as ſoon as it has rained, great care is taken to meaſure exactly all the water which is gathered in this earthen veſſel, of which a particular regiſter is kept. This meaſure is made in a little veſſel of a cubical figure, whoſe ſides were three inches, ſo that 32 lines heighth of water in this little veſſel, are anſwerable to half a line's heighth on the ſurface of the great tin veſſel: wherefore there is drawn near the brim of this cubical veſſel, which has no

* Jan. 16. 1700.

cover

cover, a line within 4 lines diſtance of the brim, that in filling this little veſſel to the height of this line, there may be the value of half a line's heighth of the water which falls. This is the ſtate of what fell in each month of this year.

	Lines.		*Lines.*
Jan.	11 $\frac{1}{2}$	July	11
Feb.	11 $\frac{1}{2}$	Aug.	18 $\frac{1}{2}$
Mar.	11 $\frac{1}{4}$	Sept.	35
Apr.	36 $\frac{1}{4}$	Octob.	12 $\frac{1}{4}$
May	22 $\frac{1}{4}$	Nov.	9 $\frac{3}{4}$
June	29 $\frac{3}{4}$	Dec.	15 $\frac{1}{4}$

The ſum of the height of the water which fell in the whole year, is 224 $\frac{1}{4}$ lines, or 18 inches 8 *l.* $\frac{1}{4}$. Though this year appeared to be extremely dry, we ſee nevertheleſs that the quantity of rain-water is but little leſs than what falls in the mean years: but it muſt be obſerved, that commonly the greateſt rains happen in the months of *July* and *Auguſt*; whereas this year they happened in the months of *April*, *May*, *June*, and *September*; and that the greateſt did not exceed 36 lines in a month, which is but little in compariſon of what frequently falls in ſummer. But, in ſhort, the three firſt months and the three laſt of this year together, have only given very near as much as the months of *April* and of *September* together. We may obſerve, that the moſt plentiful rains this year happened in one and the ſame day, and were not continual, and that very conſiderable intervals of time paſſed without rain.

As to the barometer, which is placed at the top of the great hall of the obſervatory, and near 20 toiſes above the river, the greateſt height of the quickſilver therein was but 28 inches 3 lines,

lines, the 21 of *Nov.* and the 31 of *December*; and its loweſt was but 26 inches 9 lines, on the 14 of *Jan.* and the 14 of *December*; and conſequently the difference of the heights of the quick-ſilver between the greateſt and leaſt height was 18 lines.

The thermometer, which I uſe to make obſervations of heat and cold, is placed in the E. tower of the obſervatory, which is uncovered in ſuch a manner as to be ſheltered from the wind, and that the ſun never ſhines either on the ball nor on the tube. All the obſervations which I daily make are a little before ſun-riſing, which is the time wherein the air is commonly the moſt cool. I have made obſervations on this thermometer to determine its mean height, and after having left it ſome days in the bottom of the cave of the obſervatory, I found that the ſpirit of wine therein remained at the height of 38 parts, which I take for a mean height; ſo that when it is of this height in a place where it is expoſed, I judge that the temper of the air is between cold and hot. I have found that during this year from the firſt day of *Jan.* to the 5th of *June*, there has but little variation happened in the height of the liquor, and that theſe two days it was the ſame from 42 to 43 parts, and that it froze very little, and that only in the beginning of *Feb.* the thermometer not falling lower than 29 parts $\frac{1}{2}$, which is commonly the time when it is moſt cold, as the greateſt heat happens at the beginning of *July*. But the 11 of *Dec.* the thermometer fell to 25 parts $\frac{1}{2}$, which was the coldeſt of the year; and the 25th of *July*, which was the hotteſt, the thermometer roſe to 63 parts $\frac{1}{2}$: by which we ſee that the heat of the ſummer, compared to the mean ſtate of the air, was near double the cold

compared

compared even with this mean ſtate, though the obſervation of the heat of the air was made only before the ſun-riſing.

I alſo obſerved the 23d day of *October* the declination of the needle to be 8° 10′ in the ſame place, and with the ſame needle with which I uſed to obſerve. This needle is 8 inches long, and is one of the beſt which was ever made; but though it was very lightly hung, I do not find in the obſervations of the ſubſequent years, the ſame progreſſion of declination; whether it be that the declination does not keep an equal motion, or that there may be any other cauſe of irregularity, which may alſo proceed from the obſervation, though I uſe all the precautions I am capable of: but it is always difficult to obſerve the minutes on an inſtrument, whoſe degree is but one line at the moſt; it will be much more difficult to obſerve them, when the degree has but half a line, ſuch as are the common compaſſes of three or four inches diameter. All that can be done in this caſe, is to take a long ſeries of obſervations, as of ten or twelve years, made with the ſame needle, and in the ſame place, and divide the difference by the number of years, and then they muſt be alſo certain, that the declination does not either diminiſh or increaſe. Example. At the end of the year 1686, I found that the declination of the needle was 4°. 30′ towards the W, and that at the end of laſt year 1699, it was 8°. 10′.———and conſequently the difference was 3°. 40′. or 220′. for 13 years, which gives for each year 17′. motion from N to W.

II. *A new manner of rendering barometers luminous, by M.* Bernoully, *profeſſor at* Groninguen, *extracted from one of his letters written from* Groninguen, *June* 19, 1707.

After having read in a little book, entitled, *A treatiſe of barometers and notiometers, or hygrometers*, the extraordinary *phænomenon* which happened in 1675, to the barometer of the late M. *Picard*, which was that interrupted light which he perceived by chance in the motion of the quickſilver, by carrying the barometer from one place to another, when it was very dark, I judged it worthy to make ſome reflections upon it; and the more, becauſe the author of this treatiſe, invites the curious to perfect this diſcovery; and ſays, that in thoſe experiments which have been already made of it, on many other barometers, to ſee if the ſame thing would happen, there was but one found which came near that of M. *Picard*: probably it might be that of M. *Caſſini*, in which M. *du Hamel* ſays there had been obſerved the ſame effect, tho' leſs viſible than in the other. I have therefore applyed my ſelf to it; and after having made ſome obſervations on this ſubject, accompanied with neceſſary experiments, the ſucceſs of which anſwered to my deſires, and conformable to the reaſoning which I had made *à priori*, I ſeem to have diſcovered the true cauſe of this *phænomenon*, and a way of making a very lively light appear in all barometers without diſtinction, at all times and in all places: ſo that here is a new kind of perpetual *Phoſphorus*, which does not conſume like thoſe made by the chymiſts.

Before

Before I explain my reaſoning to you, I will tell you that the ſame evening on which I read this *phænomenon* in this little treatiſe, I would make a tryal of it on my barometer, which had been tried about a month: I carried it then into the dark, I balanced it lightly at firſt, but without any ſucceſs, not obſerving in it the leaſt light: but at laſt having balanced it violently (which I can do without danger of breaking the tube or ſpilling the quickſilver, the tube being ſet in a piece of wood, as in a frame, and the quick-ſilver at the bottom ſhut up in a wooden box fixed to the board, and cloſe every where, ſo that it is only through the pores of the wood, that the air enters to preſs upon the quickſilver) I obſerved that when the quickſilver (riſing and falling with great quickneſs through a long part of the tube) was quite at the bottom, it caſt a very weak light, which vaniſhed as ſoon as the quick ſilver began to re-aſcend. This made me think, that thoſe conjectures which the author of the treatiſe alledges, to give the reaſon why this light had not yet appeared in any but in one barometer only, which was, that as to the others, *they had not perhaps been long enough tried*, could have no weight, ſince my barometer had been tried but four weeks or thereabouts.

After this experiment, I would try, whether the other conjectures of the author were to be admitted: he ſays, that the other barometers had not the ſame effect, *whether it was that they were not ſufficiently cleared of the air, or that the quickſilver was not pure enough.* To be certain of this, after having carefully cleanſed the quickſilver, by forcing it to paſs through the pores of a bit of leather, I put it again into the receiver, from which I drew out the air, and left

it therein 24 hours, to give it time to let the particles of air which were mixed with the quickſilver evaporate. After having thus purified it, I filled a tube with it in the common manner, with all the precaution poſſible to hinder any ſmall bubbles of air from remaining; but the barometer thus mounted had not any better effect, for let me balance the quickſilver never ſo violently, hardly could I draw that weak glimmering which ſhewed it ſelf, and vaniſhed away almoſt in the ſame inſtant.

I left the barometer in this ſtate for common uſe, having thought it a pity to take it to pieces, after having taken ſo much trouble and care in putting it together ſo exactly, that I am certain, neither in the empty part of the tube, nor among the quickſilver, there was not the leaſt particle of groſs air.

I then concluded from this ſecond experiment, that the other conjectures of the author of the treatiſe, were no more valid than this; or at leaſt, that neither the purifying of the quickſilver, nor the perfect *vacuum* of the upper part of the tube, were the principal cauſe of the appearance of this light.

I therefore ſought for the true cauſe of it, and thus I reaſoned. As the light does not appear every time that it is balanced, but when the *vacuum* is made, that is, only in the deſcent of the quickſilver, I comprehended that when the quickſilver deſcends there muſt go out of it, and reaſcend at the ſame inſtant, a very thin and ſubtile matter to occupy, and fill part of that ſpace of the tube, which the quickſilver leaves: I ſay a part, becauſe we may well believe, that the pores of the glaſs being without doubt larger than thoſe of the quickſilver (as it appears by the lightneſs of the one,

one, and the great heavineſs of the other) there enters at the ſame time thro' the pores of the tube, another matter much more ſubtile than the groſs air, but much leſs ſo than that which goes out of the quickſilver: and theſe two matters mixing together immediately, fill the ſpace which the quickſilver by its deſcent leaves to them; it is of no conſequence what names you give to theſe two matters: you may if you pleaſe, call with M. *Deſcartes*, that which penetrates through the pores of the tube, the matter of the ſecond element, or celeſtial globules; and that which is ſo fine, that it eſcapes from the quickſilver, the matter of the firſt element. In effect, M. *Deſcartes* has well enough ſhewn in his principles of philoſophy, *part* the 4th *art.* 58. that the particles of quickſilver leave among them ſuch acute angles, that they cannot be filled except by the fineſt matter, that is, by that of the firſt element.

Now you know how M. *Deſcartes* explains the production of light, making it conſiſt in the moſt rapid motion of the matter of the firſt element, aſſembled only in ſome ſpace, and in the effort which it makes on the celeſtial globules. I ſay, therefore, that while the particles of the firſt element are diſperſed in theſe little interſtices, and in a manner oppreſs'd by the terreſtrial parts of the quickſilver, they cannot acquire this rapid motion, or act or make joint efforts to produce the light, but as ſoon as by the deſcent of the quickſilver, theſe go plentifully out of it, they unite together; and being thus at once diſengaged from all other matter, they take that rapid courſe, which is common to them when they are free; and by the effort which they make on the celeſtial globules which meet them, they produce

this light; from thence the reaſon is ſeen why this light is only obſerved in the deſcent of the quickſilver; for when it reaſcends, the matter of the firſt element is ſo far from going out, that there rather enters again, a part of that which went out at its preceding fall; and the reſt is driven away with the celeſtial globules, out of the tube, through the pores of the glaſs. This is alſo the reaſon why this light always accompanies the top of the deſcending quickſilver, and is in a manner fixed to its upper ſurface; why the light produced in a deſcent is not laſting; and why on the finiſhing of each deſcent, this light is at an end, and immediately vaniſhes alſo. This proceeds from the particles of the firſt element, which were united at the going out of the quickſilver, and having made a very little way in getting diſtant from the ſurface of the quickſilver, being immediately ſcatter'd and diſperſed by the crowd of celeſtial globules, which with their impetuoſity bear them down, and thus take from them the force of producing this effect of light: ſo that it cannot laſt but in proportion, as there comes out of the quickſilver a continual and new matter of the firſt element, to ſucceed that which is alſo continually waſting, not much unlike the ſame manner in which a flame of a candle waſtes, and renews each moment. It is therefore manifeſt, that the light in queſtion cannot at the moſt laſt any longer than each deſcent of the quickſilver laſts.

I muſt now ſhew the principal, which is, why this light does not ſhew it ſelf in all barometers, and why it has not been obſerved till now except in two or three: as alſo, the way of remedying this, and making it infallibly appear in all barometers at all times, and with a ſurprizing vivacity,

vivacity, provided they do it in a very dark place, both theſe will ſtrengthen and confirm perfectly well the reaſons which I have made uſe of in the explanation which I have juſt made of the cauſe of this *phænomenon*.

I have obſerved, that if we expoſe quickſilver in any veſſel to the open air, we ſhall find at the end of ſome time the ſurface, where the air touches it, quite diſturbed and covered with a very thin pellicle, which being taken away with a clean feather, the primitive brightneſs returns on the ſurface, and it will be again poliſhed like a looking-glaſs; but if we leave the quickſilver expoſed to the air, another pellicle immediately like a cobweb, which thickens by time, will ſpread over it. And if this is well examined with the microſcope, we ſhall ſee that it very much reſembles leaf ſilver: and indeed, it is nothing but a very fine texture of a kind of moſs or moſſy hair, which is formed of little filaments, which having been ſeparated from the quickſilver, by the continual agitation of the air, and yet being not able to follow its motion, fall again with other impurities which it finds in the air, on the ſurface of the quickſilver; and intermingling by degrees, compoſe this pellicle. We obſerve the ſame thing in all kinds of liquors, which, if we let them ſtand, ſo that the air may dry the top of them, will at laſt be covered with a ſkin more or leſs thick, according to the conſtitution of the corpuſcles, which exhale from, and afterwards fall back on the liquors. All this being well conſidered, I ſay, that it is this pellicle which hinders the appearing of the light in thoſe barometers, which have been filled in the common manner. In this manner I conceive the thing. When they make the barometer, they take a tube hermetrically

ſealed at one end, and into the other they pour quickſilver, which falls drop by drop along the tube, ſo that each drop by penetrating and dividing the air from the top to the bottom, wipes it, if I may ſo ſay, and draws away with it all the impurities; which is the cauſe that in this moment employed in running along the tube, the quickſilver is more loaded with this moſs, than it would be in two or three days, if ſimply expoſed to the air. What I have here ſaid is ſo true, that if you let only one drop of quickſilver, cleanſed and purifyed as much as poſſible, fall from the height of one foot only into a veſſel, wherein there is more of it, which has alſo been ſo well purified that the ſurface thereof is ſmooth, and poliſhed like a looking-glaſs; you ſhall ſee that the drop falling on this poliſhed ſurface, will obſcure the luſtre of it in that part where it ſhall enter into the maſs of the quickſilver, and leave there a viſible ſpot, which is a certain mark that the drop, clean as it was, had been infected by the impurity of the air. It is thus that the drops of quickſilver poured into the tube, while they are running are covered with this pellicle; but by the fall of the drops one upon another, and by the preſſure of the quickſilver, theſe particular pellicles eaſily break to permit a continuity in the quickſilver; and theſe impurities not being able to agree with the motions, nor with the figure of the particles of the quickſilver, are obliged, as excrements, to retire out of the inner ſubſtance of the quick-ſilver, and place themſelves on every ſide between the concave ſurface of the tube and the convex of the quickſilver. Thus then is the whole mercurial column involved in this very thin ſkin as with a cuticle. Surely, there is great appearance

that the thing is, as I have ſaid; for the tube being filled in this manner, if it is inverted to make the barometer by ſtopping the aperture with the end of the finger, till it is immerged in the quickſilver contained in the veſſel; it will be obſerved by taking away the finger, that the quickſilver deſcending in the tube, will leave behind ſome remains of this cuticle adhering to the ſides of the glaſs on the empty parts of the tube, like the ſcum of melted lead.

It is not therefore difficult to conceive, that the barometer being made, the horizontal and upper ſurface of the mercurial cylinder may be covered with a thicker pellicle than any other part of the ſurface of this cylinder, becauſe it thickens partly by thoſe remainders which may continue adhered to the empty part of the tube, and which are at the laſt looſened, and fall again on the top of the mercurial column; and partly by thoſe which are below and are thruſt up by the weight of the quickſilver.

Therefore, for a laſt concluſion, it is ſufficient for me to ſay, that this pellicle which occupies the upper part of the quickſilver, as thin as it appears to our eyes, covers ſo well the pores of the ſurface of the quickſilver, that it ſhuts up either wholly, or in a great part, the paſſage to the matter of the firſt element, which by its violent motion alone can produce light; whence it follows, that in barometers which are filled in the common manner there will appear none, or very little, and that with great balancings, as in mine, which I have ſpoken of above, it muſt not be thought ſtrange, that ſo ſmall and delicate a pellicle can hinder the particles of the firſt element from going out of the pores of the quickſilver, or at leaſt from going out in ſuch abundance,

dance, and with ſo much vehemence; ſince we ſee daily that the quickſilver itſelf paſſes eaſily through the pores of the ſkin of almoſt all animals; but that the paſſage ſhould be entirely cloſed, if they do not ſeparate that tender web, which the phyſicians call *Epidermis*, or *Cuticula*. What contradiction is there then, that ſuch a thing may not happen in the ſubject we are upon?

Thus have I reaſoned on the cauſe of ſo ſtrange an effect. But as we are very apt to be deceived in reaſoning on phyſical ſubjects, unleſs they are confirmed by experiments, I have contrived two ways of hindering the mercurial column from being covered with this cuticle, both which have ſucceeded very well.

1. I took a glaſs tube 3 feet ½ long, open at both ends, which I took care to ſcour and cleanſe well on the inſide, that I might leave therein neither filth nor moiſture, having plung'd one end thereof a little way in quickſilver, contained in a large veſſel the moſt obliquely that the rim of the veſſel would permit; ſo that the angle which the tube made with the horizon, contained about 18 or 20 degrees; which having done, I applied my mouth to the other end of the tube, and began to ſuck; in this manner, I eaſily made the quickſilver aſcend to the top, and having even drawn ſome drops of it into my mouth, I made a ſign to one of my ſcholars, whom I had inſtructed for this purpoſe, to ſtop quickly with his finger the lower end which was dipped in the quickſilver. I muſt tell you here by the way, that I finiſhed raiſing the quickſilver by what I ſucked with one breath alone, leſt had I fetched my breath and ſucked again, there might have entered into the tube a little breath or ſpittle. The tube being

ing then filled in this manner, while my ſcholar held the lower end ſtopped with his finger, I afterwards cloſed the upper end with the cement, which I make uſe of to conſolidate broken cups or glaſſes. After having cloſed it well, I bid this ſcholar take away his finger from the end which was yet dipped in the quickſilver ; I then raiſed the tube perpendicular, and the quickſilver deſcended to its *equilibrium* as uſual ; but I had the pleaſure to ſee that it left no ſcum adhering to the empty part of the tube, as in tubes which are filled in the common manner : which I immediately took for a good ſign. Indeed I foreſaw that this would be the conſequence ; for in the manner with which the tube had been filled, it is well ſeen that the air could not touch the quickſilver in aſcending in the tube, except at the firſt drop which was a kind of ſhield, by the help of which all the remainder of the mercurial column could aſcend without ſuffering the leaſt approach of air ; but this one drop, beſides that it could not be much infected, having not divided and penetrated the air with violence, like a drop which falls, did not remain in the tube : for, as I have ſaid, I drew ſome of the firſt drops of the quickſilver into my mouth.

Thus was I ſure of having a barometer whoſe mercurial column was entirely diveſted of this cuticle ſo fatal to others. Mean while to make the experiment more conveniently without running the hazard of ſpilling the quickſilver by the removing or balancing of it, I took the tube out of the large veſſel, keeping the lower end of it cloſed with my finger, and put it into a narrower and deeper veſſel, which was half filled with quickſilver ; all being finiſhed, I impatiently waited for night, which being come, I took my barometer

thus prepared, the tube in my left hand, and the veſſel in which the lower end of it was dipped in my right hand; as ſoon as I was in the dark, I already perceived, without having yet balanced the barometer, very vivid lights, which were cauſed by a little ſhaking, impreſſed on the mercurial column by the motion of removing it: but when I began, tho' very gently, to balance the barometer, that I might give to the quickſilver a more conſiderable reciprocation than it had by the ſole motion of removing it, there appeared at each deſcent ſuch an exquiſite light, that it illuminated the neareſt objects, in ſuch a manner that I could well enough diſcern, by the help of this light, letters of a moderate ſize at a foot diſtance. I own to you, that I was very much contented to ſee the event anſwered ſo well to my expectation, and ſo much the more as it was an experiment not made by chance, but a deliberate act founded on the principles of my own reaſoning. I muſt alſo ſay, that this light appeared ſo eaſily, that the moſt inſenſible balancings, which hardly made the quickſilver riſe and fall the thickneſs of a knife, produced nevertheleſs the moſt vivid lights. The following days I repeated this experiment with 3 or 4 other tubes, which I filled in the ſame manner; but all performed their effect equally with much vivacity, and never failed; which makes me boldly advance, that all barometers thus prepared as I have ſaid, will ſhew at all times the *phænomenon*, which happened in that of M. *Picard*, and perhaps much more vigorouſly.

The ſecond way which I contrived to fill the tube with quickſilver, without the mercurial column being covered with the aboveſaid pellicle, was this in few words. I took a very clean tube,

open at one end only, which I plunged into quick-silver contained in a veſſel, which I erected perpendicularly ; ſo that there was yet nothing but air in the tube. To draw the air out of the tube, I covered the tube and the veſſel in which the open end of the tube was dipped, with a glaſs receiver made in the form of a bell, the top of which was extended into a long tail hollow on the inſide, to contain the tube, as the ſheath contains the blade of the ſword : (this receiver is made purpoſely to make theſe ſorts of experiments with the barometer.) I then applied the receiver with the tube, and the veſſel within it on the copper plate of the air-pump ; by means of which I drew the air out of the receiver, and thus at the ſame time out of the tube, which not being able to go out by the upper end which was ſtopped, went out with a little bubbling thro' the end which was dipped in the quickſilver. After having drawn the air from the receiver and from the tube, the moſt exactly I could poſſibly, I let it again enter into the receiver ; but being not able to make it again enter into the tube, becauſe of the qnickſilver in the veſſel which hindered it, by its preſſure it thruſt the quickſilver into the tube to the height of 24 or 25 inches ; ſo that it wanted but little of aſcending to the common height of the barometer ; which ſhewed that the air had been with care enough drawn out of the receiver. The quickſilver being thus raiſed, I judged that it muſt be quite ſtripped of its cuticle, ſince the very top of the mercurial column could not touch the air, except by that little which remained in the tube, but which by reaſon of its extreme rarefaction, could not alter the top of the quickſilver, and much leſs the remainder of the mercurial column, the leaſt part of which had not been ex-

poſed to the air in aſcending. In effect, when I made the experiment the night following, the light appeared in this tube with the ſame ſtrength, and in like manner as in the other which had been prepared in the preceding method. By which may be alſo ſeen, that the air which remained in the empty part of the tube could not at all hinder the light from appearing; and thus, if it does not appear in the barometers filled in the common manner, it is not becauſe they are not cleanſed from the air, but only becauſe the quickſilver contained in the tube is ſo wrapped up in this pellicle, as to ſtop the paſſage of the matter of the firſt element.

Mean while I have found by experience, that there is nothing ſo prejudicial to the appearing of this light, as humidity; for after having continued ſome weeks to balance every evening one of theſe barometers prepared according to the firſt method, to ſee if there was any difference, either in the vivacity, or in other circumſtances; and being not able to obſerve the leaſt difference to my great ſatisfaction, I contrived to pour a little water into the veſſel below, to cover therewith the ſurface of the quickſilver contained therein, and then I lifted up the tube gently, till the lower end coming out of the quickſilver in the veſſel reached the water; but as ſoon as a few drops of water were entered into the tube, I dipped it again into the quickſilver, and theſe drops aſcending to the top, covered the ſummit of the mercurial column. I was then curious to ſee if this little water would not hinder the appearance of the light: and indeed it hindered it ſo much, that with the moſt violent balancings, there was no way to produce the leaſt trace of light. I after that tried the ſame thing with rectified ſpirit

of

of wine, thinking that being of itſelf inflammable, it would perhaps rather help to produce our light then to deſtroy it: but in vain; for a few drops of ſpirit of wine had no ſooner occupied the ſummit of the mercurial column, but the light which had before appeared with all poſſible vivacity at the leaſt ſhaking of the tube, ceaſed to appear even with the greateſt balancing: from whence I conclude, that all moiſture and heterogeneous matter may either ſtop the pores of the quick-ſilver, and hinder the violent motion of the matter of the firſt element, as the pellicle does; or at leaſt partly ſtop the great rapidity with which the firſt element ought to be moved in order to excite the light: for it is viſible, that a foreign matter occupying already a little ſpace, directly above the mercurial column, where the matter of the firſt element is to aſſemble, it is, I ſay, viſible, that it cannot move in conjunction, nor conſequently with the rapidity which is common to it when it is alone, without paſſing through the pores of a groſſer matter.

III. *Remarks on the conſtruction of pendulum clocks, by M.* de la Hire *.

Altho' it does not ſeem poſſible to add any thing to the conſtruction of the great pendulum clocks as they are at preſent, to make them more perfect and juſt in the meaſure of time; yet we may make ſome remarks, which may ſerve to correct ſome inequalities that are found in them.

Firſt, it ſeems that the rod of the pendulum might be ſuſpended to a little fine ſpring inſtead of ſilk, to avoid the inconveniencies which happen to the ſilk, as of its lengthening by the

* May 22, 1700.

heavineſs

heavineſs of the weight, which is hung to it, and chiefly when the weather is dry; and on the contrary, of its ſhortening when the weather is moiſt, as it is with all threads and ſtrings compoſed of many threads twiſted together: for the ſhortening or lengthening of the ſilk, ſhortening or ſtretching out the length of the pendulum, will make the clock go faſter or ſlower. We might alſo add, that at each vibration, the ſilk makes a little twiſt at the place where it is tied to the rod of the pendulum, by the effort which the *fourchette* makes to entertain the motion of the pendulum. But the experiment which I have made for ſome years, of a ſpring applied to the rod of the pendulum inſtead of ſilk, has convinced me, that there are more conſiderable irregularities with that than with the ſilk, which I can attribute only to the different alterations of the ſpring by cold and heat, which making elaſtic bodies more or leſs ſtiff, cauſe in them very conſiderable alterations; for the ſtiffer a ſpring is, the more frequent vibrations it makes; on the contrary, when it is limber, it makes them ſlower. I was then obliged to take from a great pendulum of ſeconds, the ſpring which ſupports the rod of the pendulum, to put a ſilk to it, and I did not afterwards find all the irregularities which I had obſerved before, which convinced me entirely, that theſe inequalities were cauſed only by the ſpring.

Secondly, I conſidered, that if in the pendulum of half ſeconds, when the irregularities were not ſenſible, the little ſpring * A was fixed very faſt to the rod of the pendulum, theſe ſorts of clocks might be rendered more portable than they commonly are with ſilk, in long voyages by ſea, which may perhaps be of uſe in the knowing of

* Plate XIV. Fig. 1.

the

the longitudes; for in the common motion of a veſſel, the pendulum ſupports itſelf, and will not be ſo much interrupted in its vibrations as when it is only ſuſpended by a ſilk.

But if inſtead of a ſpring A, which is applyed by its breadth according to the length of the *arbor*, the pendulum was ſuſpended to a ſtiff and firm rod, the breadth of which ſhould cut the *axis* of the *arbor* perpendicularly, and it ſhould be ſtopped by the *arbor*, then the motion of the pendulum would entirely govern the motion of the clock.

In ſhort, if in the pendulums of ſeconds, to avoid the accidents of the ſilk and the ſprings, the pendulum was ſuſpended, as I have juſt ſaid, to a firm and ſtiff rod, which was engaged and held faſt in a little particular *arbor* * A, which moves itſelf freely upon its pivots, the *arbor* of the pallet which would bear the *fourchette*, would give the motion to the pendulum by the means of this *fourchette*, which being governed by the vibrations of the pendulum, would rectify the motion of the clock. I am perſuaded that this manner of applying the pendulum to the clocks, will be better than thoſe which are in uſe. The *axis* of the pivot A muſt be in the ſame right line, with that of the *arbor* which bears the pallet to avoid the friction of the *fourchette*, along the rod of the pendulum, which muſt neceſſarily happen to the pendulums, which have cycloids; and this friction cauſes a conſiderable inequality in the motion of the pendulum, when the oil in this place thickens, or it gathers a little ruſt. It might be objected, that the vibrations of the pendulum, which might be of unequal lengths, would not be reduced all to equality, there being no cycloid in this conſtruction. To this I

* Fig. 2.

anſwer

anſwer, that thoſe who have a great while managed with care the great pendulum clocks, know well that theſe vibrations are ſo equal, that they have no need of cycloids, and eſpecially if theſe vibrations are not of a long extent: but when we would have regard to the inequalities of the vibrations of the pendulum in this conſtruction, I ſhall explain hereafter the manner in which they muſt be corrected.

Some pendulums have been made, the *fourchette* of which was placed above the pallets, and met the rod of the pendulum below the weight of the pendulum, in a part of the rod which paſſes beyond the weight: but this conſtruction is faulty, and cannot ſerve for great clocks to make obſervations; for the vibrations being very ſhort, the weight of the pendulum has but little motion, and it may be ſtopped and turned very eaſily by a very weak power; thus the unequal motions of the *fourchette*, communicating with the motions of the pendulum, it cannot be rectified, nor the clock be juſt, as ſome of our academicians then obſerved, in comparing theſe clocks with thoſe we commonly uſe. But after the great clocks, which ſerve for celeſtial obſervations, were reduced to a point of perfection, to which one would not have believed they could ever arrive, ſince we often ſee for ſeveral days together, that they do not vary from the equal motion which they mark, one ſingle ſecond of time; there only wanted to be found the invention of reducing to a regularity, thoſe which are commonly carried in the pocket; and to regulate the motion of the balance, which is generally very unequal, a way was thought on to apply a little ſpring, which in bending and unbending might rectify the motions of it. This

principle

principle of regularity is founded upon this, that the vibrations of the ſprings put into motion are nearly equal; at leaſt when theſe vibrations are very near of equal extent.

At firſt they applied to theſe clocks a little ſpring quite ſtrait, which being held faſt by one of its extremities, bore at the other extremity which was free, a little *fourchette* which governed one of the *radii* of the balance meeting it in a mean diſtance from its centre; and by this means the motion of the balance, was reduced to a ſort of equality by the regularity of the vibrations of the ſpring. But as the length of this little ſpring could not be at moſt above $\frac{2}{3}$ of the diameter of the watch, it muſt needs be extremely weak to have its vibrations nearly equal to thoſe of the balance, and if it was ſo weak, it could not regulate the inequalities of the balance, which muſt make about four vibrations in a ſecond of time. They have therefore quitted this ſtrait ſpring, to ſubſtitute one of a ſpiral figure, which takes but little room, tho' it is of a conſiderable length. The extremity the fartheſt diſtant from the eye of this ſpiral is held faſt in two places to be there ſupported, and the other extremity toward the eye, is alſo fixed in the *arbor*, which bears the pallets and the balance, that the motion of the ſpring, which is entertained by the motion of the balance, may at the ſame time communicate its regularities to it; but when we carefully examine the motion of this balance regulated by the motion of the ſpiral plate, we perceive that there yet remains a great deal of inequality in it, ſo much that the motions of the pallets acting againſt the extremity of the ſpiral plate, and there making an effort in proportion to the length of the ſemi-diameter of the *arbor*, at the place where this

ſpring is engaged, they ſubdue the ſpring, which yields and gives way to the inequality, becauſe it is weak and ſlight, and cannot reſiſt the motion of the balance; which is eaſy to underſtand, by what I have already explained above in ſpeaking of the great pendulums ſuſpended to a ſpring.

I then conſidered that the motion of the ſpring muſt neceſſarily be applied at a diſtance ſomething conſiderable from the centre of the balance, to be able to govern its unequal motions with more force, and not to receive the inequalities of it. It was neceſſary therefore to cut the ſpiral ſpring pretty far from the eye, and to apply a little *fourchette* to this extremity, which was engaged in one of the *radii* of the balance, at about a quarter part from the circumference of the centre; and the balance was to be placed in ſuch a manner, that when the ſpiral is at reſt, and not conſtrained by the balance, the *radius* where the *fourchette* is engaged, was already perpendicular to the line traced by the point where the *fourchette* is applied, when the ſpring bends and unbends, that the *fourchette* might have leſs friction along the *radius* of the balance, as may be ſeen in *Fig.* 3.

* But here is yet another way to apply a ſpring to the balance of a common watch. This ſpring muſt be thin and broad as the common ſpirals are, but its figure is waving with the waves very cloſe, by this means we have a great ſpring which takes up but little room in length, ſo that its vibrations may be ſlow, without filling a great ſpace. The waves are formed according to the breadth of the plate, which is placed upon the third wheel, which makes it ſupport its great length eaſily; one might even give it much

* Fig. 4.

more

more breadth than to the common ſpirals, that it may ſupport itſelf more eaſily. I have proved that theſe ſort of ſprings bent into waves, have very great advantages above the other ſprings, in their being very ſoft, very long, and making but little effort in each of their parts. They have yet a very great advantage above the wire-ſprings, which have alſo length in a little ſpace, in this, that theſe ſprings in their prolongations and contractions, cannot extend this ſpire without the wire of which they are compoſed, or made, twiſting or untwiſting, which ſoon ſpoils them in a continued motion, which does not happen to the waved ſprings, even when they are made with a wire inſtead of a plate, as I have here ſuppoſed.

To make application of this waved ſpring to the balance of the watch, it muſt be held faſt by one of its extremities to the upper plate of the watch, in the ſame manner with the ſpiral ſprings; and by the other extremity where the waves end, it bears a little *fourchette*, which paſſes into one of the *radii* of the balance near a quarter part of the diſtance from the centre, as may be ſeen in *fig.* 4.

As for the manner of ſhortning or lengthening this ſpring, that it may make its vibrations either longer or ſhorter, it muſt be done as in the ſpiral ſprings, by the means of a little bridle faſtened to a piece which runs in a groove, to draw it nearer or farther from the place where the ſpring is fixed to the plate of the watch.

We may make this ſpring long or ſhort, according to the number and ſize of its waves, to make its vibrations agree with the alternate motion of the balance, for theſe two motions muſt

* Fig. 4.

be very nearly equal, that the one may entertain the other more eaſily.

See now how we may rectify the motion of the pendulum, when there is no ſilk to ſuſtain it, and the rod which ſuſtains the pendulum is held faſt in the pivot upon which it is moved, as I have before propoſed.

Let * AB be the length of the pendulum, from its point of ſuſpenſion A, to the centre B the total weight, which I ſuppoſe to be that of oſcillation, whoſe motion muſt be made by a cycloid, as M. *Huygens* has laid down. From the centre B with the *radius* BA, I deſcribe the circle AD, and having biſected AB in E, I draw EF perpendicular to AB, which may be the baſe of a cycloid AGF, which has for a diameter of its generating circle the magnitude EA, which will be the axis of it.

Now let there be AH the curve deſcribed by the evolution of the cycloid, beginning the evolution at the top A, and let there be the cycloid BIF deſcribed by the evolution of the cycloid AGF, beginning the evolution upon the baſe at F.

I ſay that the pivot, which muſt ſuſtain the rod of the pendulum, muſt be hollow under the circular figure AD, and that it muſt move upon the convex part of the curve AH. I ſay nothing of the manner of applying this pivot, and that which ſupports it, for there is no difficulty in it, and it does not ſignify whether the concave part rolls or ſlides upon the convex AH, ſince the point B will always be found in the cycloid BIF.

The demonſtration of this conſtruction depends upon the nature of the curves deſcribed by evolution. For it is certain, that if the right line AB

* Fig. 5.

rolls

rolls in applying itſelf upon the cycloid AGF, its extremity B will deſcribe the cycloid BIF, and its extremity A the curve AH, and that this line AB will be different in all its poſitions, as IGH perpendicular on both ſides to each curve BIF and AH.

But when any point as D, of the circle AD, ſhall be applied upon the curve AH at the point H, the circle and the curve will touch in the ſame point H, and the line HGI drawn from this point H for a tangent of the cycloid in G, being perpendicular to the curve AH and to the circle, will be equal to BA by the deſcribing of the circle; and conſequently the extremity I of this line will agree with the point B, placed in I upon the cycloid, altho' the rod AB is found in ſome other poſition, as AR; for it is no matter what point D of the circle AD with reſpect to the point A, where the rod of the pendulum is applied, is found in H upon the curve AH, ſince all theſe lines are *radii* of the ſame circle.

But as this ſort of hollow pivot may ſeem difficult to be excuted, I ſay that it may be made of an infinite number of different figures convex or concave, and ſuch as you will, which determines at the ſame time the figure of the curve of the hole, or ſupport, on which it moves, which will be eaſy to do, by the methods that I have explained in my treatiſe of epicycloids.

XI.

IV. *An extract of some letters written from* Portugal *and* Brazil, *by M.* Couplet *the son, Abbot* Bignon, *president of the royal academy of sciences.*

The longitude of Lisbon.

Among many observations of the satellites of *Jupiter*, which I made at *Lisbon* in 1698, there was one, which was likewise observed at *Paris*, in the royal observatory by M. *Cassini*. It was an immersion of the first satellite into the shadow of *Jupiter* on *May* 7, 1698, as follows.

It is well to observe beforehand, that I placed myself upon St. *Katherine*'s mount, situated to the S. S. W. of the town, in a convenient place for these sort of observations; that I was perfectly assured of the state of my pendulum, and that I made my observation with a telescope of 17 feet, and exactly of the same force with that which M. *Cassini* used at *Paris* for his.

May 7, 1698. The total immersion of the first satellite into the shadow of *Jupiter*, observed

	h	′	″	
At *Paris* at - -	11	9	21	P.M.
At *Lisbon* at - -	10	17	30	
The difference of the merid. is	0	51	51	
Which comes to - -	12	57	45	

by which *Lisbon* is more easterly than *Paris*.

Supposing the longitude of *Paris* 21° only, as there are strong presumptions to make it, that of

	°	′	″
Lisbon will be - -	8	2	15

This difference of the meridians between *Paris* and *Lisbon*, is much greater in *Sanson*'s maps, which have passed without contradiction for the best

beſt theſe thirty years. *Lisbon* appears in them more weſterly than by our obſervations by $52'\ 15''$ of degrees, which are equivalent to 16 or 17 leagues in this parallel.

On the contrary the new ſea-charts, printed by order of the king ſix years ago, make *Lisbon* leſs weſterly than our obſervations by $27'\ 45''$ of degrees, which is above 9 leagues. Thus the diſtance between the meridians of *Paris* and *Lisbon*, marked in the new ſea charts, is different from that of *Sanſon*'s maps $1'\ 20''$; which is conſiderable, being more than $\frac{1}{10}$ of their true diſtance which reſults from our obſervations.

The latitude of Liſbon.

As there is no great difficulty in obſerving the latitude, it is not ſo eaſy to miſtake it, provided one has inſtruments of a reaſonable ſize. The obſervations that I have made of the latitude of *Lisbon*, will ſerve to confirm that which is marked in the new ſea-charts. As for the latitude of *Lisbon*, marked by *Sanſon* in his particular map of *Portugal* in 1654, at $38'\ 27''$, it is ſo far from truth, that I do not believe any body can be miſtaken in it; alſo in a general map of *Spain* which he has made ſince, he has increaſed it all at once $2'$.

I obſerved the polar ſtar at the end of *December* 1697, with an inſtrument of 1 foot $\frac{1}{2}$ *radius*, furniſhed with glaſſes with threads, and I found at *Lisbon*,

The

	°	′	″
The greateſt meridian height of the polar ſtar - - -	41	5	40
The leaſt height of the ſame - -	36	28	0
The difference of the two heights is	4	37	40
The half of this difference is - -	2	18	50
Adding this half difference, with the leaſt height of the polar ſtar, we ſhall have for the apparent height of the pole - - -	38	46	50
From which ſubſtracting the ſuitable refraction - - -	0	1	25
There remains - - for the true height of the pole at *Lisbon.*	38	45	25

The declination of the needle at Liſbon.

Dec. 26, 1697. I obſerved at *Lisbon* the declination of a magnetical needle 6 inches long by the means of a meridian line, which I had drawn ſome days before with great exactneſs, and I found it to be $4^{\circ}\ 18'$ N. W.

The difference of the length of the pendulum at Liſbon *and at* Paris.

Before I left *Paris*, I had regulated my clock at the royal obſervatory in *July* and the beginning of *Aug.* 1697, and I had put it to the mean motion, where it had remained a conſiderable time to be aſſured of it. Having left it in the ſame ſtate, I ſet it a going at *Lisbon* the *Nov.* following, and I obſerved, that it loſt $2'\ 13''$ in 24 hours. As I had found that in raiſing the little weight as high as it could, it did not accelerate the motion of the pendulum an entire minute, which was not ſufficient; I determined to

ſhorten the pendulum, and after many attempts, I found that the pendulum of the clock ſhould be ſhorter at *Lisbon* than at *Paris* by 2 lines $\frac{1}{2}$.

The latitude of Paraiba.

As the ſky was often cloudy, I was above a month in regulating my clock, becauſe there wanted a great many obſervations to have the length of the pendulum, and during this I took the height of the ſun ſeveral times; from whence I conclude, that this town is at $6^{\circ}\ 58'\ 18''$ of ſouthern latitude.

The declination of the needle at Paraiba.

May 20, 1698, having firſt carefully drawn a meridian line, which I uſed for aſtronomical obſervations, I obſerved the declination of the magnetical needle to be $5^{\circ}\ 35'$ N. E.

The difference of the length of the pendulum at Paraiba *and at* Paris.

When I arrived at *Paraiba* in *March* 1698, my firſt care was to regulate my clock, and to put it exactly into mean motion, as well to know the difference of the length of the pendulum, as to prepare myſelf to make the obſervations of the ſatellites of *Jupiter* for determining the longitude of this town. At firſt, I put the pendulum again into the ſtate where it was when I left *Paris*, and ſet it a going, I found that it loſt of its mean motion $4'\ 12''$ in 24 hours. I then ſhortened the pendulum ſeveral times, and at laſt having regulated it upon the mean motion, I found that the pendulum muſt be ſhorter at *Paraiba* than at *Paris* by 3 lines $\frac{2}{3}$.

I afterwards put the ſame clock in the ſtate where it was, when I uſed it to make my obſervations at *Lisbon*, where I had regulated it upon the mean motion, and I obſerved that in this ſtate it loſt at *Paraiba* $2'\ 5''$ in 24 hours.

Altho' the difference which is found between the two pendulums of ſeconds taken at *Paraiba* and at *Paris*, is, as we have obſerved, but 3 lines $\frac{2}{3}$, which is not conſiderable in a length of 3 or 4 feet, as were thoſe which we compared; nevertheleſs it cauſed a ſenſible error, (as it is eaſy to ſee, ſince we know that the times employed in the vibrations of the pendulums are between themſelves as the roots of their heights) by which we ſee, that if at *Paraiba* we made uſe of a pendulum of ſeconds, like that at *Paris*, that is, 3 feet 8 lines $\frac{1}{2}$, inſtead of 3 feet 4 lines $\frac{5}{6}$, which it ſhould have in this part of *Brazil* to ſwing the ſeconds. Then its motion would be ſlackened, that in the ſpace of an hour it would not give more than about 3585 oſcillations inſtead of 3600, which it gave at *Paris*, which is near $15''$ difference in an hour; and on the contrary, if the pendulum of ſeconds of *Paraiba*, that is 3 feet, 4 lines $\frac{5}{6}$ was put into motion at *Paris*, where it ought to be 3 feet, 8 lines $\frac{1}{2}$, it would accelerate and give 3615 vibrations in an hour, inſtead of only 3600 which it gave at *Paraiba*.

Theſe obſervations, added to thoſe which have been made by many of the learned, ſufficiently confirm, that the nearer we are to the equator, the more we muſt ſhorten the pendulum; but the relation between theſe different contractions, which do not follow the proportion of the different latitudes, with which they agree, is hitherto unknown to us; altho' many able philoſophers have

have endeavoured to explain it to us; and to arrive at it, there must be yet a great number of observations on this subject, which altogether may perhaps at last, by their numerous comparisons, discover to us the true cause which has been long sought for.

The attention that we must have to know the true length of the pendulum, proper for the place where we observe it, is not confined to astronomical observations, but is also essential in an infinite number of things; as for example, in the gauging of current waters, in the practice of which we commonly use the simple pendulum, whose length is measured, as we know from the centre of the ball to the point of suspension, and this length of the pendulum, proper for the place where we are to make observations, must be measured with exactness, since, that spring, for example, which at *Paris* would supply 3600 inches of water in one hour, determined by the mean of the pendulum of the true length, that is, of 3 feet, 8 lines $\frac{1}{2}$ would seem to supply 3615 inches, if we use there a pendulum of 3 feet, 4 lines $\frac{5}{6}$, such as we must at *Paraiba*, thus we shall be mistaken 15 inches of the running in each hour.

I cannot cast my eyes upon these astronomical observations that I made at *Paraiba*, without recollecting an accident which happened to me at the same time. As I do not believe any author has ever spoken of the same thing, it will perhaps be agreeable to find it here. There is in *Brazil* a sort of serpent, about 2 feet long, and 3 or 4 inches round, called by the *Portuguese cobra de los cabeças*, or two headed serpent; not that it really has two heads, as I found after having examined it carefully, for it has only at the end of the tail a thickness, which has at a distance the

appearances of a head. The *Brazilians* or *Mazombes*, and after them the *Portugueze*, have taken it for a head ſo much the more eaſily, becauſe they extremely feared this ſort of ſerpent, for they ſuppoſe there is no cure for the ſting of it. They know even that it is dangerous to touch it after it is dead, and probably it is this which has kept them from examining it. They told me, that only the touching of it would bring the itch; I neglected this wholeſome advice, which I looked upon as an effect of their fear, but I was puniſhed for my boldneſs; for having killed ſeveral of theſe ſerpents, I flayed ſome of them to examine them, and preſerve the ſkin; and 2 or 3 days after, I ſaw myſelf really covered with puſtules, which were filled with a red water: they remained a long while, and even three months after, I was not entirely free from them.

There are in the country ſerpents of an extraordinary ſize, I killed one with a gun in the woods between *Paraiba* and *Pernambouc*, which was above 15 feet long, and 16 or 18 inches round; it was all covered with ſcales, black, white, grey, and yellow, which altogether made a very beautiful appearance: the bite of theſe ſerpents is venemous, nevertheleſs the *Brazilians* and *Blacks* made no difficulty of eating the fleſh. This ought not to appear more ſtrange, than that which is obſerved in the *manihot*, the meal of which is the moſt common food in *Brazil*, and the juice is a poiſon, as I have tried upon a dog, which I made drink leſs than half a tavern-glaſs of it at eight o'clock at night. I obſerved him for ſome time, without perceiving any ſenſible alteration. I ſhut him up that night, and the next morning I found him dead.

V. *A general method of throwing bombs in all caſes propoſed, with an univerſal inſtrument for this purpoſe, by M.* de la Hire *.

This † inſtrument conſiſts only of a ſemicircular plate ABD, which has a rule or tail BE fixed upon the edge of the circle, which anſwers to the centre C, and of which the ſide BE being prolonged, meets the centre C of the circle, and is perpendicular to the diameter ACD of the ſemicircle. The ſemidiameter CD of the ſemicircle is divided into 9 equal parts, and upon CE there are alſo marked the ſame diviſions, as upon CD. Each of theſe diviſions repreſents 100 toiſes, and they may be ſubdivided by little points into other leſſer parts; but it will be eaſy to judge by the eye alone, which is ſufficient for theſe ſorts of operations. Through the diviſions of the diameter 7, 8 and 9, draw ſemicircles with their centre in C.

To the centre C of the inſtrument, is fixed a rule CF, which is ſlit in the middle, that it may let the pin G run in it, with the head turned toward the plate; the middle of this ſlit anſwers to the centre C of the ſemicircle. This pin G paſſes alſo into the ſlit of another rule IH like the firſt, ſo that theſe two rules may be held faſt by the ſame pin, with a little ſcrew with ears, to what length, and in what angle you pleaſe.

At the extremity H of the rule IH, there is a little round plate, the breadth of the rule, which ſhould be of ſome white metal, as ſilver, upon which is marked a black point, which anſwers to the middle of the ſlit of the rule. Upon this

* July 24, 1700. † Plate XIV. Fig. 6.

rule

rule IH, from the centre H of the ſilver pin are alſo marked the magnitudes C 7, C 8, C 9.

The rule CF, which may turn freely upon its pin C, which is placed in the centre of the ſemicircle, may be held faſt in what poſition you pleaſe, upon the plate of the ſemicircle, by the means of a ſcrew at the end of the pin, which locks the plate and rule together, it muſt be obſerved, that theſe pins C and G muſt be pointed at their ends, and raiſed a little above their ſcrew.

Along the ſemidiameter CD, there is upon the plate a ſlit, into which paſſes or runs a ſliding piece O, with a pointed head, to which is faſtened the thread of a plummet: the middle of this ſlit muſt be upon the diameter ACD of the ſemicircle.

As for the uſe of this inſtrument, it may ſerve at firſt to determine the diſtance from the ſtation where the mortar is, to the mark, without its being neceſſary to know the number of toiſes of this diſtance. This is done by taking ſome baſe of 3 or 4 hundred toiſes on the ſide of the place where one is, and aiming at ſome known object at this diſtance, by the pin C and the ſliding piece G; aim alſo at the mark by the pins C and G, fix the rule CF, in this poſition on the plate, by means of the ſcrew at C. Afterwards remove the inſtrument to the ſtation where you aimed at firſt, and then place the ſliding piece O at the number of toiſes from the point C to the other ſtation: and aiming alſo by the ſliding piece and by the pin C, make the pin G run upon the rule CG, till you ſee by the ſliding piece O and the pin G, the mark you aim at: then ſtop the pin G in this poſition, and the diſtance CG will be in the parts of CO the number of toiſes from the ſtation where you ſhoot, to the mark; which is not neceſſary to know,

provided the pin G is ſtopped upon the rule CF in this poſition. But if this diſtance was given, you need only place the rule CF upon the line CE, and ſtop the pin G in the number of toiſes there marked.

To have the elevation or depreſſion of the mark, with relation to the horizon of the place from whence you are to ſhoot, the plate muſt be held faſt in ſuch a ſituation, that the thread of the plummet P be applied to the point A, or upon the diameter AD, and move the rule CF, ſo that you may ſee the mark by the point of the pins C, G. You muſt then ſtop the rule CF very faſt upon the plate of the ſemicircle. Theſe are the two things we muſt always know, together with the force of the powder in whatſoever method it be.

As for the force of any certain quantity of powder, or of the height to which the caſt can be raiſed in aiming toward the zenith; as it cannot be known by experiment, we know by the demonſtration that the point of the horizon where the bomb may come, when it is caſt in an angle of 45°, or half a right one, is alway diſtant from the place where you aim, double the elevation of the vertical caſt; this is what is called the amplitude of the caſt; wherefore one ſingle obſervation made in this manner, and with its conveniencies, may ſerve for all ſorts of caſes. For example, if the caſt is 1600 toiſes, the vertical or perpendicular caſt will be raiſed to 800 toiſes. This inſtrument may alſo ſerve to make this caſt, as will be ſeen hereafter.

Now for the practice of caſts, and the uſe of the inſtrument, you muſt place the ſliding piece O upon the number of toiſes of the vertical caſt, as in the figure, upon the point of 900 toiſes,

toiſes, if the vertical caſt is 900 toiſes, or elſe if the amplitude of the caſt be 45°, 1800 toiſes. Afterwards move the rule IH in its groove upon the pin G immoveable upon CF, till the ſame diviſion of 900 toiſes of this rule HI meet upon CE, when IH will be pretty near parallel to AD, or IH perpendicular to CE; which will be known by turning the rule IH, if its diviſion of 900 touches the line CE. Then if you turn the rule IH upon the pin G, the length GH remaining always the ſame, that you found it. When the centre of the plate H ſhall touch the circle OLL of 900 toiſes, ſtop the rule IH very faſt to the rule CF with the ſcrew which is at G. Now if you apply one of the ſides of the tail BE, which we ſuppoſe of equal breadth every where, within the mortar or cannon, and afterwards raiſe or depreſs it as much as is neceſſary to make the thread of the plummet P paſs by the point L, the caſt will fall at the mark deſigned.

If the point H cannot meet its circle but in one point, there is but one ſingle caſt, which can go to the mark; but if it can meet it in 2 points, theſe 2 points, as L, will ſerve to make 2 different caſts in the ſame manner, that we have explained for one, which will both go to the ſame mark. If the point H cannot meet its circle, there is not any caſt which can go to the mark.

The demonſtration.

* The demonſtration of this practice depends upon a propoſition of the parabola, which I have demonſtrated, and which M. *Blondel* relates from me in Chap. 7. of part III. of his treatiſe. For the point G being the mark aimed at, and by

* Fig. 7.

my

my conſtruction GH being equal to the difference, between the height of the vertical caſt CO, and the height of the mark G above the horizon, which paſſes by C, or elſe the ſum of the ſame, if the point G is below the horizon; or under the level of the point C, that is, if GH is perpendicular to the horizon, the line OH will be the level: therefore the points of interſection LL of the two circles OL, HL, are the foci of the parabola's, which will paſs by C and G, and it is this figure which M. *Blondel* has only reverſed in his ſquare inſtrument, which does not alter the conſtruction.

Now I ſay that the line OL being vertical or perpendicular, will make with OH or CE of my inſtrument, which is parallel to it, the angle that the direction of the caſt muſt make to the point C, without there being need to make a new operation; and it is in this chiefly that the ſimplicity of this practice conſiſts.

For if you draw LM perpendicular upon OH, and biſect it in S, the point S muſt be the ſummit of the parabola of the caſt. But alſo by the properties of the parabola, we know that the line LC being drawn, and OC being a diameter, the tangent CN of the parabola at the point C, will biſect the angle OCL, and conſequently the angle OCN determines the angle of the caſt, if OC is conſidered as vertical. But I ſay alſo that the angle HOL is equal to the angle OCN, which is evident, ſince the 2 rectangular triangles CON, OLM, have one equal angle CON, OLM, the two lines CO, LM, being parallel, and cut by the ſame OL: therefore in the inſtrument, if the thread of the plummet paſſes by OL, it is plain that OH or CE,

or any other which is parallel to them, will give the inclination of the mortar, or the line by which the caſt muſt be made.

It may be obſerved by the caſts of bombs or bullets, that as there are in almoſt all caſts 2 points as LL, there will be alſo 2 different caſts which go to the ſame mark, but the upper one will be more fit, to make its effort againſt bodies which are placed in a level, and the other caſt will have more effect againſt bodies that are perpendicular, as walls.

Pl. XIV. *Vol. I.*

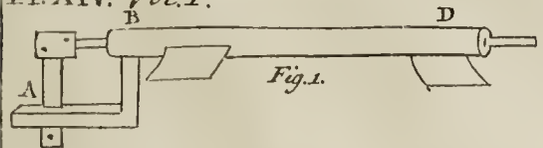

Fig. 1.

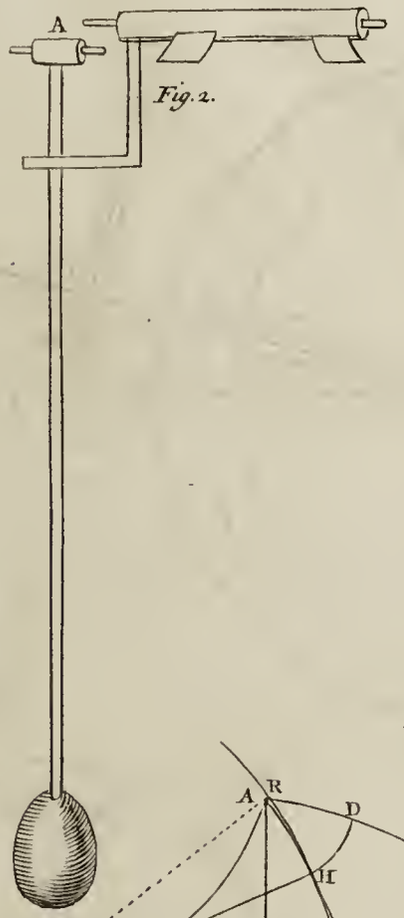

Fig. 2.

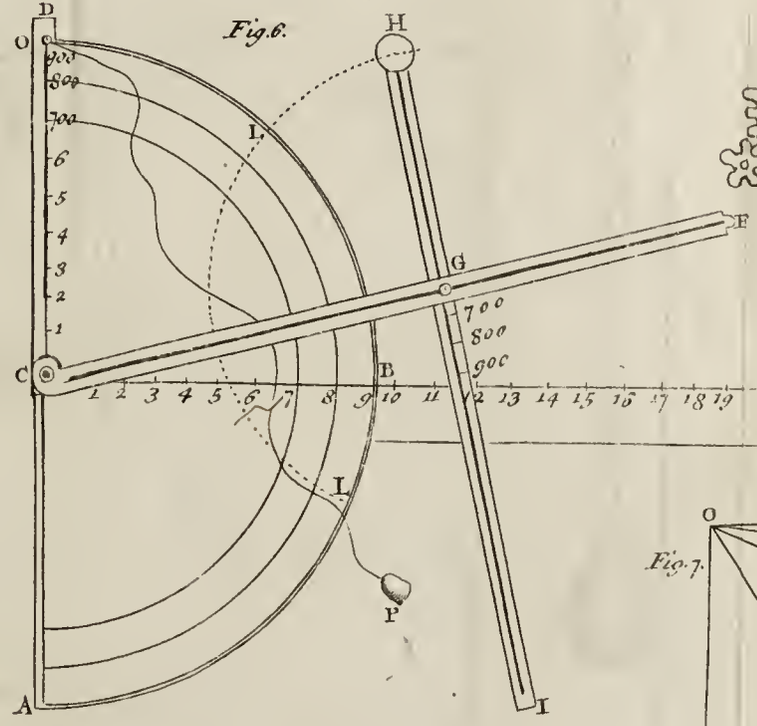

Fig. 6.

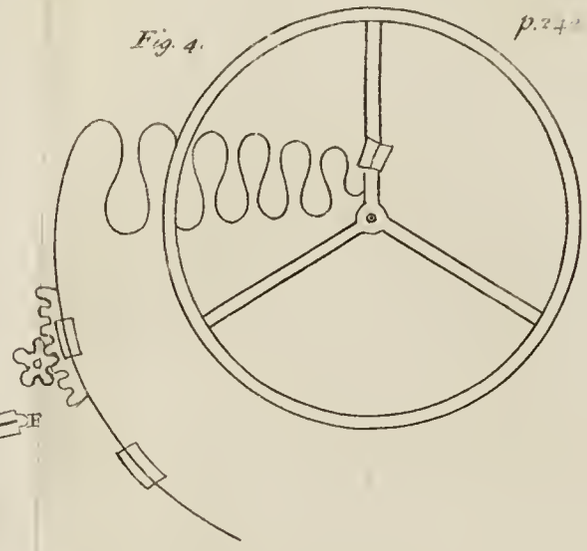

Fig. 4.

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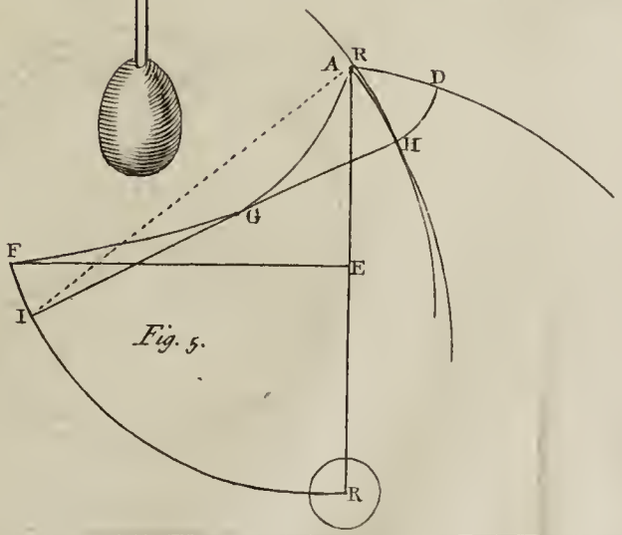

Fig. 5.

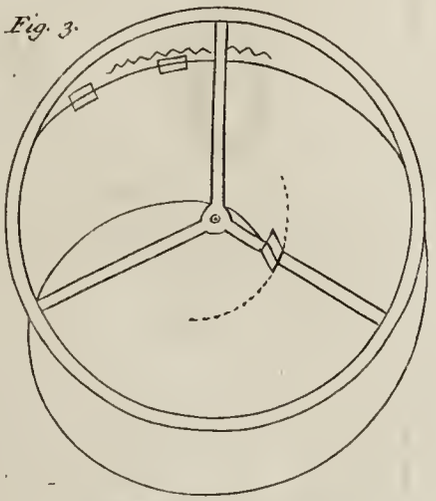

Fig. 3.

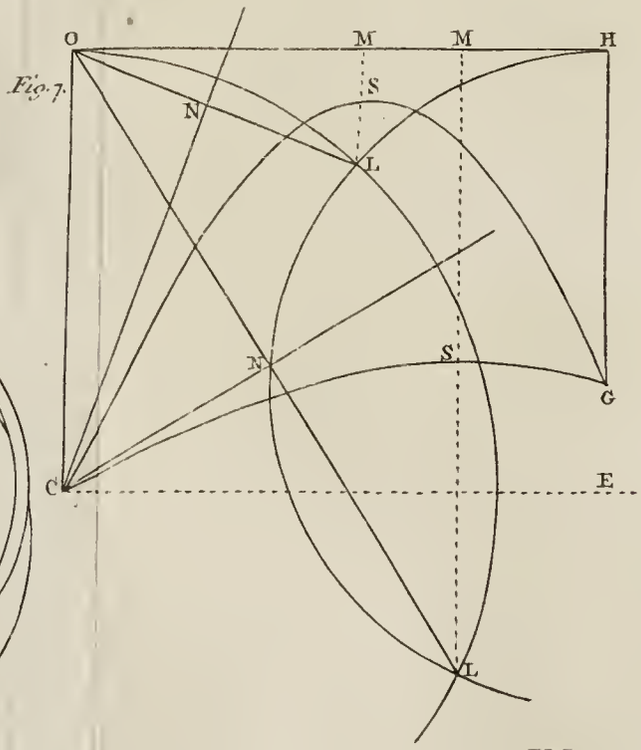

Fig. 7.

J. Mynde sc.

A

TABLE

OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCI.

In the HISTORY.

I. *ON the declination of the needle.*

II. *A method propoſed for obſerving the flux and reflux of the ſea in the ports.*

III. *On the continuation of motion.*

IV. *Of an extraordinary ſort of ants in* Surinam.

V. *Of a black ſand found in* Italy.

VI. *Of another ſort of ſand found in the mountain of* Piſaro, *which being viewed by the microſcope, appears like precious ſtones.*

VII. *Of the fatal effects of the damps in a well at* Rennes.

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IX. *On the poſition of the* axis *of windmills, with regard to the wind.*

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I. *Of the new* phoſphorus, *by M.* Bernoully, *profeſſor at* Groninguen. *Extracted from one of his letters written at* Groninguen, Nov. 6, 1700.

II. *Obſervations on the rain-water, which fell at the royal obſervatory in* 1700; *with ſome remarks on the thermometer and barometer, by M.* de la Hire.

III. *A letter from M.* Bernoully, *professor at* Groninguen, *concerning his new* phosphorus.

IV. *Remarks on the measure and weight of water, by M.* de la Hire.

V. *Anatomical observations made on the ovaries of cows and sheep, by M.* du Verney, *jun.*

VI. *On the circulation of the blood of those fishes, which have gills, and on their respiration, by M.* du Verney, *sen.*

AN

AN ABRIDGMENT OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS, in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1701.

I. *On the declination of the needle; tranſlated by Mr.* Chambers.

DR. *Halley*, a learned *Engliſh* philoſopher, having made a voyage to the ſouthern parts, has, among other philoſophical treaſures, brought with him a general theory of the variation of the loadſtone.

In the vaſt ſea which ſeparates *Europe* and *Africa* from *America*, he finds four ſeveral places wherein the needle does not decline at all.

Firſt, In 18°—½ of weſtern longitude and 2° of northern latitude.

Secondly, In 4° ——— of weſtern longitude. and 37°½——— of ſouth latitude.

Thirdly, In 10°½——— of weſtern longitude, and 16°¾ ——— of ſouth latitude.

Fourthly, In 64°——— of weſtern longitude, and 31°½——— of north latitude.

The longitudes being all reckoned from the meridian of *London*.

Having theſe four points, he imagined that they might be comprehended in a curve-line, which ſhould ſurround the terreſtrial globe, and under which the needle ſhould have no variation, but in all places on one ſide of it the variation ſhould

ſhould be eaſtern, and in all thoſe of the other weſtern.

This notion, ſo new and pleaſing to the mind, on account of that order which it introduces into a matter, whereof there was before no appearance, would be ſtill further deſirable for the improvement of navigation in long voyages, where it is very inconvenient to be in a perpetual diſtruſt of the needle, and not to dare intirely to credit any *calculus* depending thereon.

Dr. *Halley* had the ſatisfaction to find, that all the other obſervations in his whole voyage agreed with and confirmed this ſyſtem, the declination being always found eaſtern or weſtern, and greater or leſs, according as the places whereon this or that ſide of the curve line exempt of all variation, and as they were more or leſs diſtant therefrom.

But M. *Caſſini* the younger, being employed in continuing the meridian of *Paris* thro' the ſouthern provinces of *France*, and obſerving at the ſame time the ſeveral declinations of the loadſtone in the ſeveral places, did not find them altogether ſuch as they ſhould have been by Dr. *Halley*'s ſyſtem; for continuing the lines, which that author had drawn on the ocean thro' the intermediate lands and *Mediterranean* ſea into the continent of *Europe*, it appeared, that the declination, for inſtance, in the gulph of *Lyons* is 2° — greater than it ſhould be by the new doctrine — But the reaſon may be, that the laws of variation undergo ſome change between the ocean and ſuch diſtant continents, and it would be well worth our attention to obſerve this defect of uniformity, and find the meaſure and quantity thereof; the leaſt we can do is to favour, as far as nature will allow

us,

us, ſo beautiful a diſcovery, and never to give it up till we can no longer hold it.

If it be true, that the line of no variation will be movable on the ſurface of the earth, ſince the variation changes in the ſame places at the rate of 11 or 12 minutes in a year; but it may be added, that this variation ſeems confined within certain bounds, for in the whole courſe of time that the needle has been known to point towards the north, it has altered ſo little as to leave the obſervers till of late in the error of believing it directed preciſely to the north; and upon the whole, 'tis not improbable, but that the motion of the line of no variation is included between a kind of tropicks.

II. *A method for obſerving the flux or reflux of the ſea in the ports, tranſlated by Mr.* Chambers.

Tho' the tides have paſs'd for a myſtery almoſt impenetrable to the human mind, yet the cauſe thereof ſeems at length diſcover'd; but what will appear ſurprizing, we have more room to flatter ourſelves upon having the true ſyſtem, than upon exact obſervations of its *phœnomenon*. The academy therefore bethought themſelves of procuring ſuch obſervations from different places, and accordingly a memoir was drawn up by Father *Gouye* and M. *de la Hire*, upon the hints and ſuggeſtions of the whole company, and preſented to the co unt*de Pontchartrane*, who having the direction of marine matters, ſent it into all the ports of *France*; its tenor is as follows:

A method of obſerving the flux and reflux of the ſea in ſea ports.

1. A place is to be pitch'd on well ſhelter'd, where the ſea has no other motion beſides its flux and reflux; here a ſtake is to be planted, ſurpaſſing the greateſt height to which the water riſes in ſuch place.

2. This ſtake to be divided into $\frac{1}{2}$ inches, reckoning from the ſurface of the ground, and parallel lines to be drawn thro' each diviſion.

3. At each tide it is to be noted in a journal, what diviſion of the ſtake the ſea was at, when either higheſt or loweſt, and if this happen to fall between 2 diviſions, the interval to be eſtimated as nearly as may be.

4. The hour and minute when the ſea appear'd either higheſt or loweſt on the ſtake, is likewiſe to be obſerved by a well regulated watch.

5. If the ſea at low-water retire from the ſtake, it may ſuffice to mark the diviſion it is at when higheſt, and the time thereof.

6. Let it be obſerved, as often as may be, at what preciſe time the ſea arrives at the ſame deviſion both riſing and falling.

7. Obſerve what winds blow while the ſea riſes and falls, and in what direction the tide drives both riſing and falling.

8. Obſerve the croſs wind of the road, as alſo that which ranges the entrance of the port.

Laſtly obſerve a few times in a year, whether there be not a little reſt between the times, when the ſea riſes and falls, in order hereto it will be proper to moor a ſloop in the road in calm weather, and a-croſs the edges of the ſloop to place a little wooden axis fitted to turn readily, at each end, whereof may be a little turn-pike whoſe

arms

arms to enter 5 or 6 inches in the water, between the 2 contrary motions of the ſea, it will be eaſy to obſerve whether this machine do not remain ſome time, without turning, and in caſe it do, how long ſuch ceſſation continues.

III. *On the continuation of motion; tranſlated by Mr.* Chambers.

The firſt notions of phyſicks, for inſtance, the eſſence of matter, and the nature of motion, tho' the moſt ſimple in themſelves, are not yet the cleareſt, and thoſe principals which it ſeems ought to be perfectly known before we go farther, remains ſtill in great meaſure unknown, and we advance nevertheleſs.

Why a ſtone thrown upwards by a hand, continues to move after the hand has quitted it, is no eaſy queſtion, and perhaps may be longer e'er it be ſolved, than the cauſe of the ebbing and flowing of the tide.

Deſcartes maintains, that motion is a kind of being, which by its nature ſhould always continue as well as reſt, and that the ſtone once put in motion, would always perſiſt therein, if it did not communicate its motion to other bodies it meets withal, and ceas'd at length to have any motion itſelf, by this continual communication of it to others.

An author whoſe diſſertation on the ſubject is printed in the memoirs of *Trevoux*, diſapproves this ſyſtem of M. *Deſcartes*, and after combating it very ingeniouſly, ſubſtitutes another in its place.

He alledges, that at the time when the hand moves, and raiſes itſelf with the ſtone, a column of air deſcends to take the place of the lifted hand, that the motion of this deſcending column

muſt be accelerated like that of all other bodies, that this acceleration of motion, continues after the cauſe of the motion has ceas'd acting, and conſequently, that the ſtone when it has quitted the hand, continues riſing not by vertue of its being impell'd by the hand, but by the reaſon the air which takes its place continues to deſcend.

But M. *de la Hire* hereupon obſerved, that a heavy body only weighs in a fluid it floats in, by reaſon ſuch fluid is lighter than the body, conſequently that a maſs of air does not weigh in air of the ſame nature, that if it move and fall therein 'tis by the impulſe of ſome foreign power; that it is no ſooner abandon'd thereby than it ceaſes to move; and laſtly, that there is no room for acceleration, ſince it is not the weight that acts.

M. *Parent* likewiſe attack'd the new ſyſtem, by a great number of difficulties, which ariſe from it. What for inſtance ſhould be the cauſe of horizontal motion, ſince here is no acceleration of falling air? and why, when the motion is both horizontal and circular, as when a bowl, faſten'd to a horizontal rod, is ſtruck perpendicularly to the rod, why here does the bowl turn round, ſince the air is not moved circularly, but only in a right line; if the ball come of the rod it would continue moving according to the tangent, to the point of the circular circumference where it was then found; did the air deſcribe this tangent or rather all the poſſible tangents, in which the bowl would continue moving if it came off, and what ſhould be the cauſe of reflection on this hypotheſis? The air you will ſay, which goes before the moving body, being reflected upon meeting the obſtacle, carries the body back with it; but if the obſtacle were

removed

removed as ſoon as the air has made its impulſe, ere the body be arrived, would it be reflected notwithſtanding?

IV. *Of an extraordinary ſort of ants in* Surinam.

M. *Homberg* received a letter dated *Jan.* 24. 1701, from *Paramaribo*, a *Dutch* ſettlement in the province of *Surinam*, on the N. coaſt of S. *America*. This letter relates, that there are in that country a ſort of ants, which are called by the *Portugueze viſiting ants*, and with good reaſon. They march in troops, and like a great army. When they ſee them appear, they open all the boxes and cabinets in their houſes; they enter and extirpate rats, mice, *cackerlacs*, which are inſects of the country, and in ſhort, all the noxious animals, as if they had a particular miſſion from nature, to puniſh them and deliver men from them. If any one is ungrateful enough to anger them, they fall upon him, and tear in pieces his ſhoes and ſtockings. The misfortune is, that they do not make their proceſſions often enough. They would be glad to ſee them every month, and they are ſometimes three years without appearing.

V. *Of a black ſand found in* Italy.

M. *Geoffroy* being in *Italy* obſerved a black ſand, which they commonly uſe for writing. It is very much mixed with little flat, and ſhining particles, like enamel, all the others which compoſe it are without any gloſs. In putting ſome of this ſand near a loadſtone, M. *Geoffroy* ſaw, that it had only the little obſcure particles which fixed themſelves to it; from which he judges, that they

are iron or loadſtone; but he is ſure they are not loadſtone, becauſe iron does not draw them at all. As for the ſhining particles, he believes them to be a black talky powder, and indeed they find talc in many places in *Italy*; and M. *Geoffroy* has obſerved, that about *Rome*, when the ſun begins to appear after rain, all theſe little atoms of talc, which have juſt been waſhed, glitter, and have a very agreeable effect.

The ferruginous particles of the black ſand thrown into the fire, do not flame at all, as filings of iron do. This comes from their being half vitrified, and cloathed with a very earthy bitumen, which ſerves them for a varniſh, and defends them from the action of the fire.

The great quantity of black ſand which they find in *Italy*, is upon the ſurface of the earth as a ſort of indication of its compoſition within, and the volcanos which make ſuch ravages there, depend on the ſame cauſe with theſe ferruginous particles, mixed with a little bitumen, and half vitrified.

VI. *Of another ſort of ſand, found on the mountain of* Peſaro, *which being viewed by the microſcope appears like precious ſtones.*

Another ſand heaped upon the mountain of *Peſaro*, drew alſo the curioſity of M. *Geoffroy*. It is by its extreme hardneſs very fit for working perſpectiive-glaſſes, for it reſiſts a great while at this work, whereas the other ſand turns very ſoon to a powder ſo fine, that it does not grind any longer upon the glaſs, and they are obliged to change it very often. This ſand of *Peſaro* is mixed with little particles, ſome as clear as cryſtal,

tal, others as green as emeralds, others like amethyſts, topazes and jacinths; and when we view this duſt with a microſcope, it is a ſurprizing collection of precious ſtones. There is however a conſiderable quantity of particles of iron, as in the black ſand of *Rome*.

VII. *Of the fatal effects of the damps in a well at* Rennes.

There is in the city of *Rennes*, near the gate *Morlaix*, a well made three or four years ago, into which a maſon let fall his hammer. A labourer, who would fetch it up again, going down, was ſuffocated in approaching to the water. A ſecond, who went thither to draw up the dead body, had the ſame fate; and likewiſe a third. At laſt, they let down a fourth, half drunk, and well tied, whom they had charged to cry as ſoon as he perceived any diſorder. He cried as ſoon as he was near the water, and they drew him up quickly; but he died three days after. They let down a dog, which cried at the ſame place, and died three days after having been drawn up. When they threw water upon the dog while it was dying, he recovered, like thoſe which are thrown into the famous *Grotti di Cane* near *Naples*. The three bodies were drawn up with hooks and opened; but they could not find any cauſe of their death. What is very ſurprizing is, that this is not new ſtirred earth, which is the cauſe of theſe fatal accidents; and that they drink every day the water of this well without inconveniency. F. *Louvard*, a benedictin of the abbey of St. *Denys*, received this account from *Rennes*, and communicated it to M. *Varignon*.

VIII.

VIII. *An improvement in the art of dialling.*

When we make a dial, we muſt begin by finding what the poſition of the plane is, for example, of the wall where we would make it, with regard to the ſun, and the principal circles of the heavens. At firſt the compaſs preſents itſelf for this uſe; but experience ſoon undeceives us. The iron which is almoſt every where in buildings turns the magnetick needle irregularly, and gives it another declination than that which we know it ought to have, and upon which we reckon; and if it had only its natural declination, the compaſs is too ſmall an inſtrument, to give exactly the diviſions and the parts of a degree, which will be neceſſary to the juſtneſs of the operation.

We had then recourſe to 2 or 3 points of ſhadow taken upon the plane of the dial. They ſerve to determine the poſition of it, and to find afterwards by the theory of gnomonicks all the lines that we would repreſent. It is upon them that all the juſtneſs of the dial depends. They are taken in the ſame day at 3 or 4 hours from one another. They muſt be taken as diſtant as poſſible, becauſe all the other points, and the intermediate lines are afterward more diſintangled.

To have theſe points as diſtant as they can, there muſt be two things: 1. They muſt be taken in the ſolſtices, or within 10 or 12 days of them at moſt, becauſe the farther the ſun is diſtant from the equator, which is commonly repreſented upon the dial by a right line, the more the lines which repreſent the circles parallel to the equator, are ſenſibly curves, and conſequently their points which anſwer to the ſame hours, are farther diſtant from one another.

2. The ſtile muſt be long, for the ſhadow of its extremity makes ſo much the more way, and the points which mark the ſame hours are much farther diſtant.

But the ſeaſon of the ſolſtices, and the length of the ſtile, have their inconveniences.

It is very troubleſome to work in the air in either ſolſtice. The ſun does not often appear long enough, or often enough in a day; and indeed it is a great confinement to have but theſe two very ſhort times to make a good dial in.

It is exactly the extremity of the ſhadow of the ſtile that muſt be had, that is, the ſhadow of a ſingle point. But it is very difficult to have it exactly, becauſe the extremities of a ſhadow are always ill terminated, and uncertain, and beſides light has a certain trembling, which becomes more ſenſible at the extremity of the ſhadow of a greater body. Therefore the late M. *Picard*, and M. *de la Hire* have each invented a different plate, to take more exactly the end of the ſhadow of the ſtile. The deſcription of it may be ſeen in M. *de la Hire*'s treatiſe of gnomonicks, printed in 1698.

But however ingenious theſe methods may be, M. *Parent* has obſerved, that with their help, it is yet difficult to have the ſhadow of the end of the ſtile very juſt, and he has thought of another method, which gives this ſhadow very exactly, and at the ſame time renders the conſtruction of the dial independant of the ſolſtice. Beſides, the points of ſhadow, which by the common method are obliged to be taken in the ſame day, may be taken at very diſtant times, even at ſix months.

* M. *Parent*'s method conſiſts in the uſe of an inſtrument DBHFG, which is a kind of frame,

* Plate XV. Fig. 1.

of

of which the two ſides BH, FG, are ſtrongly joined and parallel between themſelves. FG muſt be about two feet long, and there put a teleſcope which has two threads croſſed at its *focus*. Upon the ſide HBD, in its whole length BD, it muſt have at the face oppoſite to the teleſcope a groove, exactly parallel to the axis of the teleſcope. BD muſt end in a point D, which muſt be in a right line with the groove.

Chooſe in the night any fixed ſtar. The obſerver ſees it with the teleſcope FG, and at the ſame time carries the inſtrument DBHFG upon his ſhoulder, the ſide HBD being that which lies upon the ſhoulder, which determines the diſtance of two parallel ſides of the inſtrument to be only one foot at moſt. The obſerver muſt have engaged in the groove the end of the ſtile that he deſigns for the dial, and at the ſame time turn his back to the wall where he would draw it. Conſequently the point D will be in the air, and in this ſituation, not loſing ſight of the ſtar, which he ſees at the interſection of the threads, he walks by degrees backward toward the wall, till he ſtrikes it with the point D. It is viſible, by the conſtruction of the inſtrument, and by the nature of the operation, that this point D marked upon the wall, is the very exact extremity of a ray conducted from the ſtar to the wall by the end of the ſtile, and we ſee at the ſame time by this practice what are the principal attentions required, both by the conſtruction of the inſtrument and the operation.

In like manner we take a ſecond or third point of ſhadow of the ſame ſtar, but at as diſtant hours as we can. It does not in the leaſt ſignify that it be in the ſame night, becauſe the motion of the fixed ſtars being very ſlow, in proportion

to

to that of the ſun, we ſhall find it again at the end of ſix months, in the ſame ſenſible poſition, whereas we ſhall not find the ſun again from one day to another.

The ſtar muſt be taken as near the equator as it can; it is plain that the points of ſhadow at different hours will be moſt diſtant.

To be more ſure of the operation, you may take with different ſtars the points of ſhadow that you have occaſion for. If the lines that are to be drawn by the means of theſe points, are found to be the ſame in uſing the points of different ſtars, the operation has been made with all poſſible juſtneſs. If they are not the ſame, there muſt be others drawn which keep exactly the middle.

In adding to the inſtrument upon BD, a groove oppoſite and parallel to that which is abſolutely neceſſary, we ſhall put it in a ſtate of proving itſelf. For you may turn it upon the two oppoſite faces of the ſide BD, and in theſe two ſituations ſee the ſame ſtar, the end of the ſtile being always engaged in one or other of the grooves. And if you find always the ſame point upon the wall, the two ſides HBD, GF, are in an exact paralleliſm, which is all the perfection of the inſtrument.

To this new invention, which is only for the practice of dials, M. *Parent* has added two others which regard the theory.

There is a portable dial very ingenious, and very uſual in compaſſes, invented by M. *de Vauleſard*. It is an azimuthal one joined with a horizontal. It has very great advantages, but it is always determined to a certain height of the pole, and is uſeleſs out of that. M. *Parent* having ſtudied a great while to remove this

fault, and to render it univerſal, is at length arrived at it. M. *Parent*'s new dial has alſo the convenience of not having need of any foreign meridian, and to find out the eaſt itſelf.

But it is true that it is charged with a great number of lines, of which many are curves, and difficult to deſcribe. M. *Parent* has therefore thought of another, which preſerves all the advantages of the firſt, is univerſal and finds the eaſt itſelf, and with all this is only rectilineal.

VIII. *On the poſition of the* axis *of windmills, with regard to the wind.*

A windmill, conſidering merely the 4 ſails which turn by the impulſe which they receive from the wind, is a more ingenious machine, depending upon much finer principles than we think, and whoever ſhall reflect upon them for the firſt time, will ſee that it is not very eaſy to ſay why the mill turns.

In order to this, we muſt ſuppoſe the theory of compound motions. A body which moves perpendicularly againſt any ſurface, ſtrikes it with all the force that is in it; if it moves parallel to this ſurface, it does not ſtrike it at all; in fine, if it moves and meets it obliquely, this motion which partakes both of the perpendicular and the parallel, and is compounded of them, has no effect upon the ſurface, but ſo far as it is perpendicular, and according to the proportion of what perpendicularity it has to what it has of paralleliſm, and drives the ſurface only according to the perpendicular direction, which enters into its compoſition. Thus every oblique direction of a motion, is the diagonal of a parallelogram,

of

of which the perpendicular and parallel directions are the two ſides.

Again, when a ſurface, which being obliquely ſtruck, has received only the perpendicular impreſſion, is faſtened to ſome other body, in ſuch a manner that it cannot follow this perpendicular direction, but ſome other which approaches more or leſs to it, then the perpendicular itſelf becomes the diagonal of a new parallelogram, one of whoſe ſides is the direction, that the ſurface ſtruck may follow, and the other ſide that which it cannot follow. Thus, when a rudder faſtened obliquely to the keel of a veſſel, is ſtruck by the current of water parallel to this keel, and conſequently it is ſtruck obliquely by it, we ſee that in drawing upon the ſurface the line of the perpendicular impreſſion, that it tends to diſengage itſelf from the keel, and to get from it, and that this direction perpendicular to the rudder, is oblique to the keel. It would get from it then by an oblique motion: but as it cannot diſengage itſelf, nor conſequently get from it, it muſt only take, in this oblique motion, that of the two directions which compoſe it, by which it may be moved without getting from the keel, and leave as uſeleſs that which would have removed it. Now the direction, according to which it may move without getting from the keel, is that which makes it move circularly about its extremity as a centre; then all the effect of the oblique ſtroke of the water upon the rudder is reduced at firſt to a perpendicular impreſſion, which is again reduced to make the rudder turn, or, if the rudder is immoveable, to make the veſſel turn.

In an oblique and compound motion, where only one of the directions is uſeful, the greater *ratio* the other has to it, the leſs effect will the

motion have, and on the contrary. In examining the compound motions of the rudder, we ſee that the more oblique it is to the keel, the *ratio* of the direction, which ſerves to turn it on the other, is the greater; but, on the other ſide, the more oblique it is to the keel, and conſequently to the courſe of the water, which is ſuppoſed to be parallel to it, the weaker it is ſtruck. The obliquity of the rudder has then at the ſame time an advantage and a diſadvantage: but as they are not equal, and as each of them is always varying with every different poſition of the rudder, they are differently complicated; ſo that it is ſometimes one, and ſometimes the other which prevails, and that more or leſs; it has been a queſtion to find the poſition of the rudder, where the advantage ſhould be greater than in any other poſition. M. *Renau*, in his famous theory of the working of ſhips, has found, that the rudder, to be in its beſt ſituation, muſt make an angle of almoſt 55 degrees with the keel.

If a windmill, expoſed directly to the wind, ſhould have its four ſails perpendicular to the common *axis* to which they are fitted, they would receive the wind perpendicularly, and it is viſible that this impreſſion would tend only to overturn them. It was therefore neceſſary to make them oblique to their common *axis*, that they might receive the wind obliquely. Let us conſider only one vertical ſail. The oblique impreſſion of the wind upon this ſail is reducible to a perpendicular direction. This direction, which cannot be intirely followed by the ſail, is compoſed of two, one of which tends to make it turn upon the axis, and the other to fall backwards. But only this firſt direction can be followed, and conſequently all the impulſe of the wind upon the ſail has no other effect, than

to

to make it turn from right to left, or from left to right, according as its acute angle turns either way. And the invention of this machine is ſo happy, that by the ſame reaſons the other three ſails are determined to turn the ſame way.

The obliquity of the ſails with reſpect to their *axis*, has exactly the ſame advantage and diſadvantage, with the obliquity of the rudder to the keel; and M. *Parent* having ſtudied by the analyſis the moſt advantageous inclination of the ſails upon the *axis*, found it to be exactly the ſame angle of almoſt 55 degrees. Probably the experience of thoſe who have invented mills made them find this angle after long trying: but as experience teaches us, that theſe common and groſs practices are by no means infallible, and that they often are wide of the mark, it is well to examine them by geometry, and be aſſured if what we do is the beſt that can be done.

M. *Parent* has even ſearched if there was nothing to correct in the common practice of putting the axis of the mill exactly in the direction of the wind. Theſe are not eaſy inquiries. We muſt find, by mechanicks, the proportion of all the different forces, which enter into the motion of a machine, according to all its different diſpoſitions poſſible, and having given them algebraic or analytic expreſſions, we muſt by the ſame analyſis find of all theſe expreſſions that which determines the greateſt force. By this learned circuit, M. *Parent* has arrived only to juſtify the common practice. The axis of the mill muſt be put in the direction of the wind; and they were put ſo before we were ſure that it was right.

AN

ABRIDGMENT

OF THE

PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris* for the year 1701.

I. *Of the new* phoſphorus, *by M.* Bernoully, *profeſſor at* Groninguen. *Extracted from one of his letters, written at* Groninguen, Nov. 6, 1700.

THE objections, which ſome gentlemen of the academy have made to my diſcovery of the light of the barometer, have been the occaſion of my making a new one, of much greater importance and curioſity: it conſiſts in making of the quickſilver a portable and perpetual *phoſphorus*, which I can carry about, and ſend conveniently and without danger whereſoever I will, and at all times; for it will laſt for ever, and yet its light, to the ſtrength of which that of the barometer is not to be compared, never diminiſhes. As this diſcovery, tho' very eaſy, as you will ſee hereafter, has been drawn from the principles by which I have believed, and ſtill believe, that I have explained the light of the barometer. I am verily perſuaded, that I am not ſo much miſtaken in my judgment, as you may perhaps think, with thoſe gentlemen, who attribute the light of my barometer to the nature of my own quickſilver; for you ſaid not a word upon the explication which I have given; it is apparent, that you have taken it for a conceit, ſeeing that my experiment done

done again by ſome of your gentlemen, had not the wiſhed ſucceſs; and that on the contrary, the barometer of M. *le Marechal de Villeroy*, that my Lord *Portland* gave him, produced light even after having been filled with his quickſilver in the common manner. Theſe are objections I own, which ſeem to deſtroy all my reaſonings. I ought then to anſwer them, if I would ſupport my invention: but what ſhould I ſay, if I had been preſent at theſe experiments: I ſhould perhaps have taken notice of a hundred circumſtances, in which they had not well obſerved my method of filling the tube: for example, the manner of filling it by the means of a leathern purſe, which you ſay is equivalent to mine, has however this difference, that here it is the quickſilver that muſt drive the air before it, which in making ſome little reſiſtance, may let ſome little remains or bubbles of air fix themſelves to the ſide of the glaſs, which will already ſuffice to engender the pellicle of the quickſilver, which I have ſaid hinders the light; whereas by the ſuction it is the outward air, which drives the quickſilver up, and which conſequently only follows the motion of the air within, which by its rarefaction goes out of the tube, as I may ſay, voluntarily. It is not that I would abſolutely diſapprove this manner of filling the barometer by the means of a leathern purſe, I imagine it muſt be proper enough for the exciting of the light. This is then another fault in the circumſtances; it may be that the tube which they uſed was not dry nor clean enough; for the leaſt moiſture or greaſe hinders the appearance of the light. This tube was alſo too ſlender, not being, as you ſay, above a line and half diameter within; the largeſt tubes are the beſt for this purpoſe, as I have known by

experience. The reaſon of it is evident; for beſides, that the quickſilver in a large tube balances more freely than in a narrow one, where the friction of the quickſilver againſt the glaſs leſſens the quickneſs of the deſcent: the pellicle which covers the quickſilver, if it makes any, muſt be more thick in a narrow tube than in a wide one, becauſe not being able to extend itſelf in breadth, it thickens in height.

As for the other experiment, I queſtion whether they were able to fill the tube of quickſilver with the mouth as I do, without ſuffering a little breath or ſpittle to get in, it being very difficult to hinder it, ſeeing others have not ſucceeded therein. It requires a particular dexterity; for my part, it is not difficult for me to do it, being able beſides by a ſort of habit to draw with my mouth out of a little receiver $\frac{7}{8}$ of the air which it contains; ſo that there remains only $\frac{1}{8}$ part, and without ſtraining myſelf too much. I have yet other conjectures, which make me ſuſpect the experiments you mention, but as I was not preſent, I can ſay nothing for certain: I hope nevertheleſs, that if the gentlemen will make a ſecond trial of my experiment, in obſerving well what I have juſt now ſaid, they will have better ſucceſs. But I had rather that they would do it at night than in the day, altho' in the dark; for you know, that if in the day one goes ſuddenly into a dark place, the eyes being dazzled with the too great brightneſs of the day, do not ſo well perceive a faint light, ſuch as is that of the barometer, tho' otherwiſe it may be briſk enough in the night.

M. *de Villeroy*'s barometer, which you ſay made the light, after having been filled in the common way, throws me I own into ſome difficulty: but I ſhould have been glad to have ſeen

it

it filled; and then I do not doubt but that I ſhould have been able to find ſome anſwer; perhaps when they threw in the quickſilver, they held the tube very obliquely to the horizon, to let the drops of quickſilver run ſoftly, as in a channel, which muſt have hindered the air from affecting it ſo much as it would have done, if they had let the drops fall in vertically with impetuoſity. In effect, after having filled a tube in this manner with my quickſilver, I have perceived the light more than ordinary; but always much leſs than by the manner of ſuction, or of the air-pump. In all caſes, why may I not ſay, that the quickſilver of M. *de Villeroy* is ſo exceeding well purified, that there is no more heterogenous matter of which the contact of the air can form a pellicle; and thus my manner of explaining the cauſe of the light of the barometer, will be rather confirmed than deſtroyed.

Beſides, if my explanation was not the true one, and it was to the particular nature of the quickſilver, and not to the manner of my having filled the tube, that the production of the light ſhould be aſcribed, pray tell me, why my ordinary barometer filled in the common manner, altho' very much ſhaken, makes little or no light; whereas the other tubes, filled in my manner with the ſame quickſilver, make an excellent light with the leaſt balancing? I made again for the ſecond time, many experiments upon it, as ſoon as I received your letter. And to convince myſelf entirely, that this light was not the effect of a ſingular property of my quickſilver, which all other has not, I borrowed ſome other, with which I made the ſame experiments again, and with the ſame ſucceſs as with my own. Now it is morally impoſſible, that the ſecond quick-

ſilver ſhould be exactly of the ſame nature with my firſt; ſince, as they pretend, it muſt be of ſo extraordinary a quality, that among a hundred ſorts of quickſilver, they ſhall not perhaps find one which makes a like effect.

To come now to the diſcovery of my new *phoſphorus*, I thought that one of the principal reaſons, by which the pellicle hindered the appearance of the light in the barometer, might be the two great uniformity of the motion of the quickſilver in ſo uniform a tube. For in riſing and falling thus the length of a cylindrical tube, its pellicle muſt never change its thickneſs, nor break, but on the contrary, remain always fixed to the upper ſurface, with which it riſes and falls without ever quitting it; in ſuch a manner, that it makes no opening, by which the matter of the firſt element, as I call it with M. *Deſcartes*, may get out of the pores of the quickſilver, after the manner mentioned in my laſt letter. But it being ſo difficult to avoid this pellicle in a full barometer, even according to my manner, I conclude, that notwithſtanding this pellicle, provided it is not too thick, the like muſt always appear, if by any means I could make it burſt, or diſperſe in pieces by the motion of the quickſilver, which makes me judge that nothing would be more proper for this purpoſe, than a very violent, irregular, and not uniform motion of the quickſilver, incloſed in a glaſs a little bigger, and of an unequal figure, from which the air has been emptied as well as poſſible. This reaſoning was confirmed by the following lucky experiment, which makes the ſubject of my diſcovery.

I took a clean fair phial, which held about half a pint, and ſtrong enough to ſuſtain the agitation of the quickſilver; I put into it 5 or 6 ounces

ounces of quickſilver well purified; after that, I cemented on the neck of the phial with great exactneſs, a cock, which I afterwards applied to the air-pump, to draw the air out of the phial; having done this as carefully as I could poſſibly, I ſhut the cock to hinder the air from entering into the phial again, when it was ſeparated from the air-pump: this is all the artifice.

To try then if I had reaſoned juſtly, I carried the phial the ſame night into the dark, and holding it faſt by the neck, I began to ſhake it ſtrongly, as they do in rinſing a bottle, to give a great agitation to the quickſilver. And preſently the phial appeared full of a fire, the light of which was not interrupted, nor intercepted, like that of the barometer; but laſted as long as the quickſilver was in agitation, and with ſo much vivacity, that I could eaſily ſee the faces of the ſpectators enough for to know them: I repeated this experiment many times, with more than one ſort of quickſilver, and always with the ſame ſucceſs; except when I had not been exact enough in drawing the air out of the phial, or that I had let a little get in again; for then not only the light appeared much fainter, but it leſſened by degrees, notwithſtanding the continual ſhaking of the phial, till it diſappeared intirely. After that, there was no way to make it appear again, without drawing the new air out of the phial.

Having examined by day, what was the cauſe of the faintneſs of the light, I found all the ſurface of the quickſilver covered with a pellicle, not only viſible, but ſo thick, that it was like a paſte made of dirt: notwithſtanding before the agitation the phial was very clean within, and the ſurface of the quickſilver poliſhed like a looking-glaſs. From whence I judge, that the air being

agitated, altho' in a ſmall quantity, might extremely infect the quickſilver, and that for this reaſon, as faſt as this paſte formed itſelf, the light grew fainter, and intirely diſappeared, when this paſte acquired ſuch a conſiſtence, as not to be broken in pieces by any violent agitations that were given the quickſilver. It is alſo by this, that you may know whether the air is well taken out of the phial or not ; for if it is well emptied, not only the light does not leſſen, and the quickſilver does not cover itſelf with dirt, but the light itſelf becomes more excellent in time ; and the pellicle of the quickſilver, if it had any at the firſt, diſperſes intirely, in ſuch a manner, that the quickſilver poliſhes itſelf ſo well at laſt, by frequent uſe, that there does not remain any ſpot ; and when this happens, then the light is in the higheſt degree of vivacity, and appears always with the ſame force every time that you ſhake the phial in the dark.

I do not believe, that they have found till now the perpetual *phoſphorus*, that is, which does not conſume by time, or at leaſt which does not in the end loſe its virtue ; but here is one which muſt laſt as long as you will without loſing any of its own, provided the phial remains always well ſtopped, and that the air does not enter ; for the quickſilver incloſed in the *vacuum* is not ſubject to any alteration, ſo that there is no reaſon why it ſhould not have its effect at all times.

It is true, that the cock which I had cemented upon the phial, with which I made the firſt experiment, began to ſpoil by the quickſilver, becauſe it was of braſs ; but I have ſince found the ſecret of ſtopping the phial after having drawn out the air, without having occaſion for the cock. Among many ways which I thought on, the moſt

moſt ſure and expeditious is, firſt to ſtop the phial before you draw out the air, with a cork and a proper wax over it, and then to make a little hole with a pin through the wax and cork, to give an opening to the air which you are going to draw out of the phial; this being done incloſe the phial in a receiver, out of which the air muſt afterwards be drawn, as exactly as poſſible, to get it at the ſame time out of the phial by the aperture of the little hole: the greateſt difficulty is how to cloſe this little hole, before you let any air get into the receiver. To perform this eaſily, you muſt expoſe the receiver thus empty to the ſun, and with a convex-glaſs melt the edges of the wax about the hole, in this manner the hole is filled with the melted wax, and ſtops itſelf perfectly well of its own accord. This being done, you may *ex abundanti*, apply the receiver again to the air-pump, to ſee, if during the operation, there has not a little air ſlipped into the receiver by ſome inviſible aperture: the ſureſt way is during the operation to hold all the parts of the receiver, by which the air can ſlip in, under water. Being thus aſſured, that all is done well, let the air enter again into the receiver to take out the phial, which being thus prepared, ſerves then for a *phoſphorus* whenever you will take the pains to ſhake the phial in the dark. I kept 5 or 6 weeks two of theſe phials filled with two different ſorts of quickſilver, which had their effect admirably well. The curious, to whom I have ſhewn them, have declared, that they have ſeen nothing more wonderful. In ſhort, the whole phial is in a flame, and the quickſilver like a burning liquor.

If you have a mind to make one of them, you muſt carefully obſerve theſe three things, 1. That

the phial be very clean and dry within; and if you doubt of its being ſo, it would be better to take a quite new one, as it comes from the glaſs-houſe. 2. That you do not ſtir the quickſilver much before the air is drawn out of the phial. 3. That you empty the air out of the phial as carefully as is poſſible. This third point muſt be obſerved with all the exactneſs imaginable, or elſe it will not make any *phoſphorus*, or if it does, it will be faint, and not laſt long. The air-pump muſt be very good; mine, which was made in *Holland*, is ſo nice, that the *piſton*, being drawn from the bottom of the cylinder to the mouth, and remaining ſo for 24 hours, does not let in the 10000th part of air into the cavity of the cylinder, as I have known by experience, a certain ſign of very great exactneſs.

II. *Obſervations on the rain-water which fell at the royal obſervatory in* 1700; *with ſome remarks on the thermometer and barometer, by M.* de la Hire *.

During the whole year 1700, there fell 20 inches of water at the obſervatory, which is the mean quantity that falls every year.

There fell in

	Lines.		*Lines.*
Jan.	11 $\frac{3}{4}$	July	35 $\frac{3}{4}$
Feb.	13 $\frac{1}{4}$	Aug.	9
Mar.	13 $\frac{1}{4}$	Sept.	1 $\frac{1}{2}$
Apr.	27 $\frac{3}{4}$	Octob.	24
May	17 $\frac{1}{2}$	Nov.	25 $\frac{3}{4}$
June	44 $\frac{1}{4}$	Dec.	16 $\frac{3}{4}$

* Jan. 8, 1700.

We

We may obſerve, that in the two months of *June* and *July* alone there fell 80 lines, which is $\frac{1}{3}$ of that which fell in the whole year: but the month of *Auguſt* gave only 9 lines altho' there are generally the greateſt rains in it, as I have obſerved for many years ſince I have made theſe obſervations. What is alſo remarkable in the rains of this year is, that there fell but 1 line $\frac{1}{2}$ in *September*, whence the drought was very great towards the end of ſummer.

The thermometer ſhewed us, that the greateſt cold of the months of *January* and *February*, was on the 9th of *February*, when my thermometer fell only to 28 parts, the mean ſtate of heat or cold being at 48 parts, as I have proved for ſome time in the bottom of the caves of the obſervatory, where my thermometer always remained at this height. This thermometer falls ſometimes in the winter below 15 parts. It is expoſed to the air, but ſhelter'd from great winds and the ſun. All the reſt of theſe two months it was commonly at 40 parts; we muſt take notice, that all the obſervations that I made every day, were about ſun riſing, when the air is commonly the coldeſt. But on the 19th of *December* the thermometer fell to 25 parts $\frac{1}{2}$, a little lower than it did at the beginning of *February*, which is generally the coldeſt time. By theſe obſervations it appears, that the cold has not been very conſiderable during all this year.

As for the heat, it was the greateſt on the 21ſt of *July*, the thermometer marking 61 parts $\frac{1}{4}$.

We ought not to judge of the coldneſs of the air, by the impreſſion which it makes on our bodies; for a great wind makes the air always appear colder to us, than it really is, becauſe it makes the air paſs through the cloaths, and drives away

away that which is about the ſkin, and which is warmed by it. This experiment proves that the violence of the wind does not augment the cold of the air. I blowed with a pair of bellows againſt the ball of a thermometer for ſome minutes, and I did not obſerve any ſenſible alteration in the height of the ſpirit of wine. This experiment would have been more juſt, if it had been made with one of *Sanctorius*'s thermometers, where the impreſſion of heat and cold is made on the air it ſelf.

For the barometer, that which I uſe is fixed at the top at the great hall of the obſervatory. The greateſt height was on the firſt day of *Jan.* at 28 inches 4 lines and more than $\frac{2}{3}$, with a gentle wind at ENE, and it always kept very high during this month, altho' the wind was often toward the W. but always inclining a little to the N. yet it was not then cold, for the air was commonly in a temperate ſtate, and ſometimes hotter, the barometer at the loweſt was at 26 inches 8 lines $\frac{1}{2}$, *November* 26 with a high S. wind and a little ſnow, which ſoon melted, the air not being cold. Thus the difference between the greateſt heights, and the greateſt falling of the quick-ſilver during this year, was 1 inch and a little more than 8 lines.

I found the declination of the needle 8° 12′ toward the W. *November* 20, of the laſt year 1700. This obſervation was made againſt the S. wall of the terraſs of the obſervatory, which I found was placed exactly according to the meridian line, and the needle which I uſed was 8 inches long, and very well hung.

III.

III. *A letter from M.* Bernoulli *profeſſor at* Groninguen, *concerning his new phoſphorus.**

I read the paper which you ſent me, but I do not know whether I ought to be ſorry for the bad ſucceſs, which it contains of the experiments upon the light of the quickſilver; I think I have rather reaſon to be glad myſelf, that no body can yet effect that which I do very eaſily, either by my ſkill, if I have any, or by the goodneſs of my pneumatic machine. For is it not ſurpriſing, that the experiments of the academy have never ſucceeded, and that mine have never failed? you ſend word, that *it is ſome particular accident, which happens to ſome quickſilver, which may render it capable of light in a place void of air*; but pray conſider that I have made my *phoſphorus* with 5 or 6 ſorts of quickſilver, which I have known were brought here from different places and at different times, which nevertheleſs have ſucceeded very well with me, except one only which did not give light at the firſt, but which I rendered luminous by waſhing it, as I ſhall mention hereafter.

I do not find it neceſſary to anſwer in order to the experiments in this paper, nor to the reflections and conſequences which are drawn from them. To end the diſpute, I deſire the academy to ſend me ſome of the ſame quickſilver, which they have uſed without ſucceſs. I engage to make a *phoſphorus* with it as good as thoſe which I have already made hitherto. And that the academy may be aſſur'd that it is the ſame quickſilver that they ſend me, with which I make the *phoſphorus*; I will do it in the preſence of authentick witneſſes, and ſend it again to the aca-

* July 5. 1701.

demy. What makes me thus bold is, that I ſee ſome circumſtances that the paper mentions, which makes me believe, that theſe experimeuts have not been made exact enough, and that conſequently the ill ſucceſs of it muſt be imputed entirely, either to the machine of the academy, which perhaps may not be the moſt juſt, or to ſome miſtake. One of theſe circumſtances is, that the quickſilver in a phial emptied of air being very much ſhaken, really gives a little light very faint, that it was ſtrong at the beginning, that it diminiſhed by little and little, but without any air entering again into the veſſel, and that after having let the air in again, and then emptying the machine immediately a ſecond time, this quickſilver had not given any more light, altho' very much ſhaken. I ſee by this, that the quickſilver had not been purified as it ought from the heterogeneous matter, of which had been formed that pellicle which I ſpoke of in my preceding, by the gathering of the air which was left in the phial, which was thought to have been emptied enough; it was by this that the light was ſo weak and vaniſhed by degrees, whereas it would have been very lively and laſting, if the phial had been well emptied, and the quickſilver well purified; for I have tried more than once, that the quickſilver incloſed in a veſſel void of air, where it ſhone very much for a long time with an equal force, has ceaſed to ſhine by loſing its light by degrees, as ſoon as I have let a little air into it. However, we may conclude from the experiments which they object to me, that the quickſilver, which was not ſhining in the barometers, for as the paper does not mention that they had different quickſilver, I believe that they uſed only the

ſame, was at leaſt ſo in a phial emptied of the air: if they had then made this experiment with the barometer alone, and had declared that the quickſilver was not luminous, would they not have been miſtaken, ſince it really was ſo? It is the ſame with the ſecond experiment, by which they did not ſee any more light in the ſame phial, after having emptied the air a ſecond time, becauſe that, according to the paper, perhaps in letting the air again into the veſſel, a little of the moiſture of the air had faſtened itſelf to the ſides of this veſſel, which they know, as well as I, to be prejudicial to the light of quickſilver. Let us ſee then what would have happened, if from the firſt time there ſlipped ſome moiſture inſenſibly into the phial; there is no doubt, that the light not appearing at all from this firſt time, it would have been ſaid, that this quickſilver was not at all luminous, altho' it had been the fault of him who had made the experiment, and not that of the quickſilver. I ſay this, that you may obſerve that the experiments which did not ſucceed, prove nothing to ſuſtain a negative propoſition; becauſe it may always be doubted in making of theſe experiments, that ſome miſtake has been committed.

I ſhall ſay nothing concerning the experiments upon the barometer, both becauſe I have already anſwered to theſe objections in my ſecond letter, and becauſe I entirely neglect making of other obſervations upon the light of the barometer, ſince I have found the way to render the quickſilver luminous in a phial; which, in my opinion, is far more curious, as it furniſhes a ſort of perpetual *phoſphorus*, and convenient to carry about, beſides that the light of it is much more lively than that of the barometer: I ſhall only ſay ſome-

thing upon the ſecond barometer, made in the pneumatic machine of the academy. They may reaſonably ſay, *that the quickſilver was not riſen to the ſame height in this laſt barometer, as it is commonly in a barometer well made, it not being poſſible to empty in this manner all the air out of the tube*; becauſe the weight of the quickſilver, preſſing upon the open end of the tube, hinders the going out of the air. I ſee by this, that they have let the open end of the tube dip too deep into the quickſilver; whereas when I made this experiment, I placed the tube in ſuch a manner, that its end was near the ſurface of the quickſilver, and very little dipped. For this purpoſe I took a veſſel, broad and a little flat, to the end that in letting in the air again, which by its preſſure had made the quickſilver riſe in the tube, this quickſilver might not fail; by this means, I raiſed the quickſilver to the height of 26 inches, ſo that it wanted but little of the common height. Altho' it be true, that it is not abſolutely neceſſary that the quickſilver be in a place perfectly void of air to become luminous, as they have very well obſerved in the paper in queſtion; however, it muſt be known that there did not remain ſo much air in the tube, as they perhaps thought: and beſides that the light, altho' very briſk the firſt night, was very much weakened the nights following, without doubt becauſe of this little air which remained in the tube, which I had not obſerved till after my laſt letter. From whence it follows, that the more perfect the *vacuum* is, the more excellent and durable the light will be; ſo that the quickſilver in a phial perfectly emptied of air (or at leaſt if it be ſo to the tenth thouſandth part or more, for it is impoſſible to draw the air intirely out of any veſſel) ſhines as lively

as

as poſſible; and very far from the lights growing fainter, it increaſes to certain degrees, and then it continues and ſhews itſelf with equal force every time that you ſhake the phial, provided alſo that the quickſilver be well purified from its dirt; I own that it is a conjecture which appears true to me, when they ſuſpected that my firſt quickſilver, which the moment it was expoſed to the air, covered itſelf with a pellicle, which became like duſt if it was ſhaken, was not ſo clean as it might be: I will agree, that this pellicle formed itſelf of a foreign matter, either metallick or other, which came from the quickſilver rather than from the air, nevertheleſs that does not at all invalidate the general explication, which I give of the production of the light of the quickſilver, nor even deſtroy my idea of the generation of the ſaid pellicle; for it is always conſtant, as I have obſerved in my preceding, that the air, if it is not always the material cauſe, is at leaſt the efficient cauſe, ſeeing that the ſame quickſilver, when it is incloſed in the *vacuum*, remains always bright and poliſhed, altho' very much ſhaken; and as ſoon as you have admitted the air, it grows thick as uſual. But I have found the ſecret of purifying it ſo well from all its dirt, that it does not thicken any more, even when it is expoſed to the air, and ſhaken very ſtrongly. I put 4 or 6 ounces of quickſilver into a phial, and pour common water upon it, till it covers the quickſilver to the thickneſs of two fingers or thereabouts; then I ſhake the phial ſtrongly for a great while, as in rinſing it, I empty the water out, which is quite black and dirty, at the top of the quickſilver, and put freſh upon it, and begin again to ſhake the phial, till the water becomes dirty again; I then change the water a ſecond time, and do the

ſame

ſame thing; which being repeated till the water is no longer black, or very little, I dry the quickſilver by paſſing it ſeveral times through a clean cloth: if you uſe ſpirit of wine inſtead of water, you will ſooner have cleaned the quickſilver. By this means I took all the dirt out of it, ſo that being ſhaken in the open air, as much as you would, it does not leave any traces of pellicle or of duſt, only it tarniſhes a little in ſome days, as if it was by the ſteam that apparently comes from the touching of the air, which is always a little infected with humidity. The *phoſphorus* made with the quickſilver thus cleanſed, was much finer than thoſe made before; which confirms my explication of the production of this light, which I have ſaid was hindered by the heterogeneous matter, poſſeſſing the top of the quickſilver.

I have ſaid above, that I have tried 5 or 6 ſorts of quickſilver, which all ſucceeded with me, of which ſome were alſo infected with duſt by the motion, like my firſt, and the others remained clear and poliſhed, like thoſe of the academy. Among theſe there was one, which, at firſt, gave no light: but I ſuſpected that, even before I made the trial; for it ſeemed to be more thick, or leſs fluid than the others, ſince, when it was agitated, it made no bubbles upon the ſurface, as I obſerved in the others, which I attributed to its viſcidity, which hindered the ſeparation of its parts. This made me ſuſpect, that there was perhaps in that quickſilver ſome oily or ſulphurous matter, which becauſe of its viſcoſity, did not ſhew itſelf upon the ſurface, as the other impurities did, which more eaſily ſeparated themſelves from the little parts of the quickſilver to be thrown out: thus this oily matter remaining always

ways mixed within the quickſilver, is taken to be very pure, tho' it is by no means ſo; and much leſs ſo, than that which covers itſelf at firſt with a viſible pellicle. Then having well waſhed it in the above manner with ſpirit of wine, rather than with water, becauſe I thought it better than water to take out the viſcocity, the quickſilver, which was not at all luminous, became as ſhining as any other: but what is ſtill more wonderful, the firſt quickſilver, which grew thick with the leaſt motion, became ſo pure with waſhing, altho' with water alone, that I have ſeen it make light even in a phial full of natural air, without its having any drawn away. It is true, that the light was not ſo lively as that which was made in the *vacuum*, and it appeared only like ſeparate ſparks, which aroſe ſucceſsfully, and periſhed almoſt at the ſame time; whereas the light in the *vacuum* is like a continual flame which laſts inceſſantly while the quickſilver is in agitation. I conclude from theſe experiments, that the quickſilver, if it is perfectly purified, may let the ſubtile matter (which I call with M. *Deſcartes*, by the name of the firſt element) go out of its pores in ſuch a quantity at once, that for all the reſiſtance of the air, it has ſtill motion enough to produce ſome light. If they then take the pains to waſh the quickſilver well, which they ſay is not luminous, and after having well dried it (for the leaſt moiſture will occaſion bad ſucceſs) pour it into a clean and dry phial; if they then draw out the air carefully, I am ſure they will ſucceed. Tho' this quickſilver is thought to be perfectly pure, becauſe it remains clear after the agitation: I have nevertheleſs obſerved, that it may be infected with a concealed glutinous matter, which ſhutting up intirely the pores of it, or at leaſt making

making them ſtiff, hinders or keeps back the particles, which ought to cauſe the light, from going out. I ſay, that the ſtiffneſs of the pores may hinder or keep back theſe particles from going out; for it is very viſible, that altho' the quickſilver be in very great agitation, if nevertheleſs the pores of its particles are not flexible enough to change their ſhape, the matter of the firſt element cannot be driven away from it, for the pores muſt often ſhrink that this matter may go out. It is therefore very likely, that the quickſilver, which is ſaid to become luminous by the diſtillation through quicklime, is one of thoſe whoſe pores are thus ſtiff, becauſe of ſome ſulphurous or glutinous matter. And therefore very far from being of the opinion with thoſe who believe they are *igneous particles*, that the quicklime has given to the quickſilver in paſſing thro' it, which produce the light; I am ſtrongly perſuaded on the contrary, that the true reaſon of it is the purification alone, ſo that the lime has contributed nothing to it but its pores, thro' which the quickſilver paſſing has left behind all the foreign and glutinous matter, and is thus delivered from it: there is then nothing done by the diſtillation than what I have done by waſhing alone. In ſhort, theſe igneous particles appear to me great paradoxes for many reaſons, of which I ſhall only mention ſome.

1. This new *phoſphorus* muſt at laſt loſe its virtue, becauſe theſe igneous particles become at laſt of no ſervice by frequent uſe, as you ſee by other *phoſphori* which are luminous, by the means of ſuch igneous particles.

2. If theſe igneous particles are ſo fine, that they can lodge in the little ſpaces of the quickſilver, and paſs through them, as is pretended, without

without doubt they could pass much more easily through the pores of the glass, which are much larger then those of quicksilver, how comes it then that in shaking the phial, they do not disperse themselves immediately by flying off thro' the pores of the glass, let them be in ever so great quantity in the quicksilver?

3. We can no more explain why the light in the barometer appears only in the descent of the quicksilver: for if it is caused by these igneous particles, which swim upon the surface, why have they not their effect when the quicksilver rises, as well as when it falls, if there wanted nothing but motion to make them successively upon the surface of the quicksilver.

Now all these difficulties are avoided by my way of explaining this light; for in saying that it is produced by a very fine matter, which being universal and found every where, never fails, we shall shew that this *phosphorus* must last perpetually, the quicksilver only lending its pores, which are very narrow, aud serve as a sieve to the matter of the first element, to separate it from the second and third, (I make use of these terms because they are convenient to explain myself) from which being delivered, and afterward driven out of the quicksilver by the agitation which is given it, it takes at first its rapid motion, which is usual with it, when it is alone and disengaged from all other matter, and so produces in our eyes the effect which causes in us the sensation of light. Therefore this *luminific* matter, altho' it is dispersed in a moment, yet is immediately followed by another which has the same effect, and so on, which will always continue. This explication proved itself also by one of these *phosphori*, with which I have made the experiment almost every

night for about a year, and I can truly ſay, that I have never obſerved any ſenſible change in it, but quite on the contrary, that the light was always equally bright, as it ſtill is; and very far from its having ſuffered any diminution, it ſeems at preſent to be a little more lively than it was at the beginning, perhaps becauſe the quickſilver is become more fluid by the frequent agitations, and the reſt of its dirt is ſeparated from it by degrees, and is fixed to the ſides of the glaſs, as the glaſs being a little thick within, plainly ſhows; ſo that the quickſilver being entirely purified, gives light for the future to the higheſt degree of vivacity. My explication appears to me in other reſpects ſo plauſible, that I am fully perſuaded, that another pure liquor as heavy as the quickſilver, if there was one, would make without doubt the ſame effect; ſo that I believe gold would be the fitteſt to make the like *phoſphori*, if we knew the way of rendering it fluid, without loſing any of its ſpecifick gravity: for as I make this effect depend on the ſmallneſs and flexibility of the pores, it is certain, that gold having the ſmalleſt of any body, it wants only the flexibility of its pores, which we cannot procure it but by a perfect fluidity of its maſs. Perhaps melted lead put into a *vacuum*, would give alſo a light, at leaſt if the dirt, with which it is always infected, does not hinder it.

As for the quickſilver, which is ſaid to be made luminous by the artificial liquid *phoſphorus*, I believe that it was not the quickſilver which was luminous, but the particles themſelves of the artificial *phoſphorus*. Now that would have happened to many other bodies, which we ſee ſhine by the friction alone of ſolid *phoſphorus*; and as the light of it does not laſt long, I imagine alſo that the quickſilver here mentioned, has not always

always kept this light borrowed from the artificial liquid *phoſphorus*; ſo neither does that light belong to our ſubject.

By all theſe reaſonings, joined to the obſervations which I have alledged, it appears that the three conſequences which they have drawn from it cannot ſubſiſt. For, 1. we ſee that the quickſilver is not only capable of becoming luminous in a place emptied of air; but that it is in effect already luminous, provided its light be not hindered by ſome foreign cauſe either internal or external.

2. Alſo all quickſilvers are equally luminous, provided again their lights are not hindered in ſome more than in others, by any cauſe whatſoever, I made a remarkable obſervation upon this ſubject: I took two phials both alike, and equal, into which I put an equal quantity of quickſilver taken from the ſame maſs; I drew the air equally and in the ſame hour from each phial. Would one not ſay, that the light ought to appear equally ſtrong in both phials, ſince in all appearance every thing was perfectly equal and alike? but the effect ſhewed quite the contrary, for one of theſe phials made the light very lively with the leaſt motion, but the other did not till after much ſhaking, and then having once began to ſhine, it wanted only the leaſt motion to make the light appear again, and after having let this phial reſt for two or three hours, it wanted to be ſtrongly ſhaken before the light began to appear again; whereas the firſt phial always gave light without any trouble. What can we ſay to this? unleſs that perhaps there entered a little moiſture into one of the phials, altho' imperceptible to my eyes, either from the breath, or from the ſweat of the hands in touching it;

which was ſufficient to cauſe ſo great a difference in the light. It does not then follow from hence, if one quickſilver gives more light than another, that it is more capable of giving it than the other; for here where the quickſilvers were taken from the ſame maſs, and conſequently equally capable of ſhining, yet they ſhined differently.

3. Laſtly, we ſee that the ſame quickſilver, if it be well prepared, muſt always be equally luminous, and that if it is ſometimes much, ſometimes little, ſometimes not at all, it is a ſign that there is an alteration happened to it, which augments, diminiſhes, or hinders the appearance of the light, which without that, would be always the ſame: witneſs the *phoſphorus* which I have mentioned above, that gave light a year without any ſenſible alteration. I can confirm what I have ſaid by another obſervation, which I made not long ago: I had a phial prepared with *phoſphorus*, which ſhone equally for about 6 weeks; without opening or letting the air into the phial, I burned with the ſun by a convex glaſs, a little bit of cork that had been ſeparated from the reſt, and ſwam upon the quickſilver. It made a little ſmoak in the phial, which otherwiſe remained in its firſt ſtate, without being at all changed. One would not believe how much this little ſmoak diminiſhed the vivacity of the light, beſides that it wanted to be ſhaken much more ſtrongly than before to make it appear; which ſhews the alteration of the light obſerved at the academy, muſt not be taken for an eſſential quality, but for the effect of an accidental cauſe.

I agree *that all common quickſilver is perfectly alike, as all gold and all ſilver is alike, from whatſoever part of the world it comes, provided that it be pure:* for this is the foundation of

my explication of this light, to maintain that it is general, and that it comes only from the quickſilver which gives way to the matter of the firſt element, to the excluſion of another more groſs matter. I agree alſo, that it follows from my explication, that all quickſilver muſt be luminous, and at all times; it is alſo both, as I have now proved, provided the light be not hindered by ſome foreign cauſe, as the ſun is always luminous, although its light may be taken from us by an eclipſe or other cauſe. I conclude then, that it is not, as is believed, a particular accident happened to ſome quickſilver, which can render it capable of ſhining in a place emptied of air; but on the contrary, that it is natural and eſſential to all quickſilver, as to all other liquids equally heavy, if we had any ſuch, to be luminous and it would be rather a particular accident happened to the quickſilver, which hindered the appearance of its light. That if after all this they ſtill find difficulties, without being able to ſucceed in making my new *phoſphorus* as perfectly as I have done it; I only wiſh to have the ſame quickſilver, which ſo obſtinately darkens itſelf; I hope to diſperſe the darkneſs, and that I ſhall have the good luck to ſupport this quickſilver in the greek title of *phoſphorus*, with as much juſtice as the planet *Venus* bears the *Latin* title of *Lucifer*.

IV. *Remarks on the meaſure and weight of water, by M.* de la Hire. *

Extract of the memoirs of M. *Picard*.

171 inches $\frac{1}{2}$ of water of *Arcueil* weigh 6 *lb.* 14 *oun.* 4 *dr.* 2 *gr.*

* *September* 2. 1701.

Hence

Hence I find that the cubical foot of water of *Arcueil* weighs 1113 *oun.* 3 *dr.* 20 *gr.* or 69 *lib.* 9 *oun.* 3 *dr.* 20 *gr.*

M. *Picard* adds that the *Paris pinte* of the measure of the *Hotel de ville* contains 47 inches $\frac{2}{7}$.

But that the *Chopine* of the *Hotel de ville* contains almost 24 inches.

And if we suppose that the *pinte* contains 47 inches $\frac{1}{2}$, it will weigh of the same water 30 *oun.* 3 *dr.* $\frac{1}{2}$, or 1 *lib.* 14 *oun.* 3 *dr.* $\frac{1}{2}$.

But if we suppose it to contain 48 inches, in proportion to the *chopine*, then we shall find it to weigh 30 *oun.* 7 *dr.* $\frac{1}{2}$, or 1 *lb.* 14 *oun.* 7 *dr.* $\frac{1}{2}$.

Aug. 5, 1701, at M. *Boulduc*'s, we weighed a *pinte* of river water in a quite new pewter *pinte* pot.

The water being warm weighed 31 *oun.* 4 *dr.*
Being cold it weighed 31 6

Aug. 20, 1701, at the *Hotel de Ville*, with M. *Boulduc*, we measured in the brass standard a *pinte* of river water; and having weighed it exactly with a good balance, after different manners, it weighed 1 *lb.* 14 *oun.* 3 *dr.* 2 *gr.* The water was neither cold nor hot.

This measure agrees exactly with that which I had concluded from M. *Picard*'s observations; for the difference of a few grains may come from the water of *Arcueil*, which M. *Picard* used, weighing a little more than the river water, and perhaps it might be a little colder than the river water, which we made use of the last time.

V.

V. *Anatomical obſervations made on the ovaries of cows and ſheep, by M.* du Verney *junior* *.

Having met with the *uterus* of a cow, which incloſed a *fœtus* of about a fortnight or three weeks, which however had all the parts very diſtinct, I applied myſelf with care to diſcover where this *fœtus* had got out of the ovary. The tubes and their expanſions appeared a little more ſwoln and ſpongy than uſual, one of the ovaries was about the bigneſs of a wallnut, a little pointed at the end, and the ſides were of a hard ſubſtance, ſet with veſicles and ſome whitiſh points. All the reſt of the ovary was of a ſpongy nature, covered with a ſmooth and very thin membrane, intermixed with ſeveral blood-veſſels. On one of the ſides of the veſicular ſubſtance, there appeared a dark yellow ſpot, of the ſize of a lentil, and I thought it might be the place where the *fœtus* got out. I blowed into it with a pipe, the air got in, and ſwelled the whole ovary, I preſſed it to make the air go out, and drove in freſh. Not only the ovary ſwelled up like a veſicular texture, but alſo a number of veſſels, which ſeemed to come out of it. I found them to be blood-veſſels. I made uſe of this aperture to enter into the ovary. I there found ſeveral cavities, very ſmooth, which appeared like little baſons. I blowed into them as faſt as they preſented themſelves, and all the veſſels, which I had ſeen before, ſwelled in like manner. The whole middle of this ovary was a ſpongy body, which was very eaſily detached; it received veſſels at its baſe, and ſome at its point.

* Sept. 7, 1701.

There

There uſually appears at this place a little dent, ſo diſpoſed, that one would always expect to find an aperture. At laſt, being willing to be ſure that I was not deceived, and thinking of a good way to diſcover the *ova* without breaking their outer covering, and how this membrane grows thin, and opens where the point of the egg is, I ſaw two ſlits at ſome diſtance from each other, very exactly cloſed by the membrane itſelf, one edge of which paſſed under the other in form of the ſcale of a fiſh.

To know whether theſe apertures are always found, I took another ovary, which ſeemed to me to be much of the ſame nature, and not being able to find them either with the pipe or probe, I made one myſelf with the lancet. But it was in vain to blow into it, for the air did not paſs either in the ovary or its veſſels. I pierced other ovaries in ſeveral places, and always in vain, which made me judge that theſe apertures are not always found and at all times. However, I have found them ſince in ſeveral ſubjects, as I ſhall mention afterwards.

To demonſtrate the *ova* without breaking their outer covering, the ovary muſt be parted into two by degrees, at the place where the veſſels enter it. Then moſt of the *ova* preſent themſelves, and we have the pleaſure of ſeeing and obſerving in what manner the membrane is thinned, the places where it is opened; and to conceive eaſily how it may be opened at the time of maturity. This did not ſeem to me more difficult to comprehend, than the manner in which moſt pods open, to let out their ſeeds.

Having opened an ovary, the bigneſs of which depending on that of the ſpongy body, I found in the inner membrane a ſlit covered by a turn

of the ſame coat in form of a ſcale, and on blowing into it, I obſerved that the air raiſed the outer membrane, and made it play.

Continuing to labour on the ſame ſubject, I found an ovary, in which the membrane of the point of the ſpongy body was open again. I was willing to be ſure whether this aperture communicated with the blood-veſſels, as in the preceding obſervation. I blowed in at the ſpermatic vein, the whole ovary ſwelled, and I ſaw the wind get out at this aperture. The ſpongy body juſt mentioned ſeems to riſe from the extremity of the veſſels, juſt as ſome muſhrooms grow upon trees, the ſponges upon briars, and the galls upon oaks, by the aperture made by the puncture of ſome inſect in ſome veſſel of theſe trees: we may ſay alſo, that the little baſon-like cavities were like cups, or the lodges of ſome *ova*, which had gotten out, with the mouths of their veſſels remaining open; perhaps at laſt they were receptacles, which were to be filled with air and ſpirits, at certain times, to give more play to theſe parts in the time of enjoyment, and to facilitate in others the *exit* of the *ova*. Theſe facts, notwithſtanding their being ſo manifeſt, were conteſted at the academy by part of the anatomiſts, and they were deſirous to ſee them upon new ſubjects. Happily I met with three *uteri* of cows, one of which incloſed a *fœtus* of about 3 weeks or a month. In another, I found each ovary covered with the hood, and embraced by the expanſions of the tube; but there did not appear any thing particular in the third.

The ovaries of the portion, which incloſed a *fœtus*, were very different from each other.

That on the ſide where the *fœtus* was, ſeemed withered, and not very veſicular, and the upper part was even, ſmooth, and of a dark yellow.

The other ovary was ſtretched, entirely veſicular, and in a manner tranſparent.

There was obſerved at the point of the firſt ovary a little aperture, ſhaped like a half creſcent, of which one return fell upon the other; I drove ſome air into it, but it did not ſwell the ovary.

At the lower part of this ovary, we perceived near an *ovum*, a little red *cicatrix*, the ſkin appeared thinned, and we diſcovered a little aperture in it, which was alſo ſhaped like a half creſcent. The wind, which was driven into it, made one of the edges riſe, but it did not penetrate quite into the ovary.

As this little aperture was at the place of the egg, where the skin is uſually found thinned, and as the air did not penetrate, I opened the ovary into two parts, at the place where the veſſels entered, and the *ovum* ſtill half covered with its cup, remained faſtened only to the place where it appeared externally tranſparent.

After having emptied the liquor, I drove in ſome air, the membrane roſe, and the air got out from between the cup and the *ovum*, by the exterior aperture, which I have deſcribed, and the wind filled the membrane of the *ovum*.

To confirm myſelf in this experiment, I repeated it ſeveral times, and it always ſucceeded.

I afterwards examined the ovaries wrapped up in the expanſions of the tube, I blowed into it, and the hood roſe. Having opened one of theſe ovaries, I found a great many fibres in it, ſo cloſely faſtened to its membrane, that, when we would have ſeparated them, they tore in ſeveral places, which made me ſuſpect this diſpoſition not to be

natural. This ovary incloſed a ſpongious body, and a great number of veſicles, the greateſt part of which ſeemed half come out of their cups; and when I blowed, the air paſſed between the membrane of the *ovum* and the cup. I afterwards opened the covering of the other ovary; on one of its ſides, the point appeared red, like a fruit of the winter cherry in its bladder; it was the ſpongy body, the point of which was extremely ſoft, and open on one of its ſides. Very near to a ſmall red cicatrix there was an *ovum* raiſed, which preſented itſelf like the point of an acorn, when it begins to come out of its cup.

When we blowed againſt the little *cicatrix*, we ſaw a ſmall membrane, ſhaped like a half creſcent, which paſſed over the place of the *ovum*, that was ſtill in the ovary. This ovary being kept for ſome days, the eggs grew withered, and when we went to preſs them, they roſe again, and partly came out of the membrane. On the other edge of the ovary there appeared alſo two other eggs raiſed.

There were ſeveral fibres of the expanſion of the tube about the baſe of the ſpongy body; ſo that we could ſee but one half of them.

After having found ſeveral other ovaries, opened at the point of the ſpongy body, I drove ſome air into them, which made them ſwell, and came out either by the blood-veſſels or ſpermatic veſſels.

There was in one of the biggeſt ovaries that could be found, a red proceſs, the point of which roſe out of the ſurface, when it was preſſed on the ſides; it was the membrane that covers the *ovum* before its getting out. It was alſo ſo empty, that, when we blowed againſt it, it was

immerged in the ovary, and eaſily ſhewed the whole cavity of the *ovum* which had got out.

In another large ovary, which I had preſerved in brandy, I obſerved that the *ova* were all withered, and that the exterior membrane ſunk inwards, in ſuch a manner that we could eaſily count them. Having dipped this ovary in water, I blowed by the ſpermatic vein, and ſaw that not only the ovary ſwelled, but that there was alſo two large *ova* among others, which ſwelled as far as their coverings could extend, theſe eggs ſeemed externally diſtinguiſhed from the reſt, by their figure and by their circumference.

All that I have ſaid here has been obſerved by me alſo in ſheep, in the beginning of their pregnancy.

An explanation of the figures.

Plate XV. *Fig.* 2. A. The ovary, upon which are ſhewn *ova* of different ſizes, which are known by their tranſparence.

B. The extremity, or the point of the ſpongy body, open at its upper part marked C.

D. D. The aperture of the expanſion of the tube.

O. O. O. O. Fleſhy fibres about the expanſion of the tube.

E. The expanſion of the tube, as it appeared when it was blown into, ſupported by the membrane, which forms the hood.

F. F. F. The tube.

G. The inſertion, or entrance of the tube into the *cornu uteri*.

H. One end of this horn.

I. I. I. I. I. I. I. I. I. The ſpermatic veins blown up by the air driven in by the natural aperture

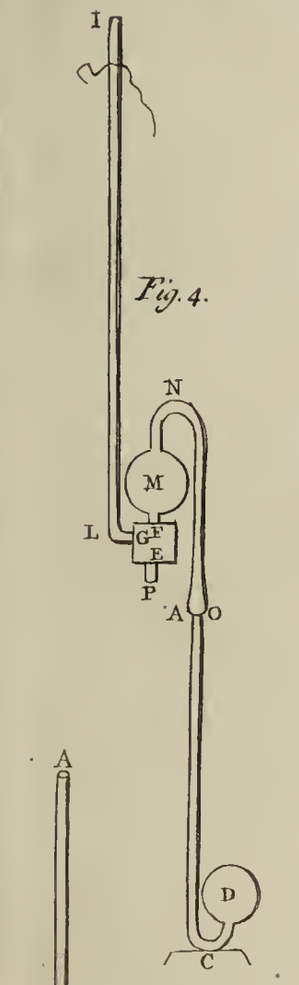

Fig. 4.

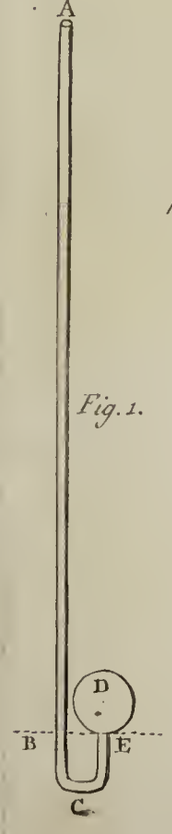

Fig. 1.

Fig. 3.

Fig. 5.

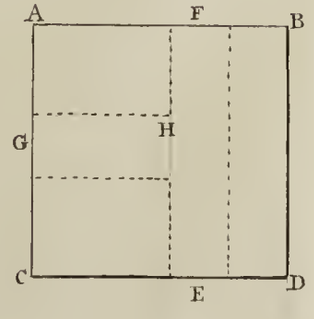

Fig. 2.

Triangulum AES, Leida. Haga. Gouda
AE. 4103. 8.
AES. 97° 11 ASE. 32° 25' FAS. 50° 23

Triangulum ESR Leida Gouda Dordracum
ES 3897. 8.
RES 25° 49
ERS 25° 49'

Triangulum EAR, Leida Haga Dordracum
AE 4103 3
FAR 85° 51 AFR 71. 31.
AR. 1012. 7 ER 10634. 7

Triangulum AEF Haga Leida Roterodamum
AE 4103 3
Ex observatis angulus EAF 39° 53'
et angulus AEF 53 40'
Unde reliquus AFE 86 27'
AF 5616 8
et FE 6972 3

Triangulum ESF Leida Gouda Roterodamum
ES. 3897. 8.
SEF 43° 36 ESF 80° 0'
EF 4883

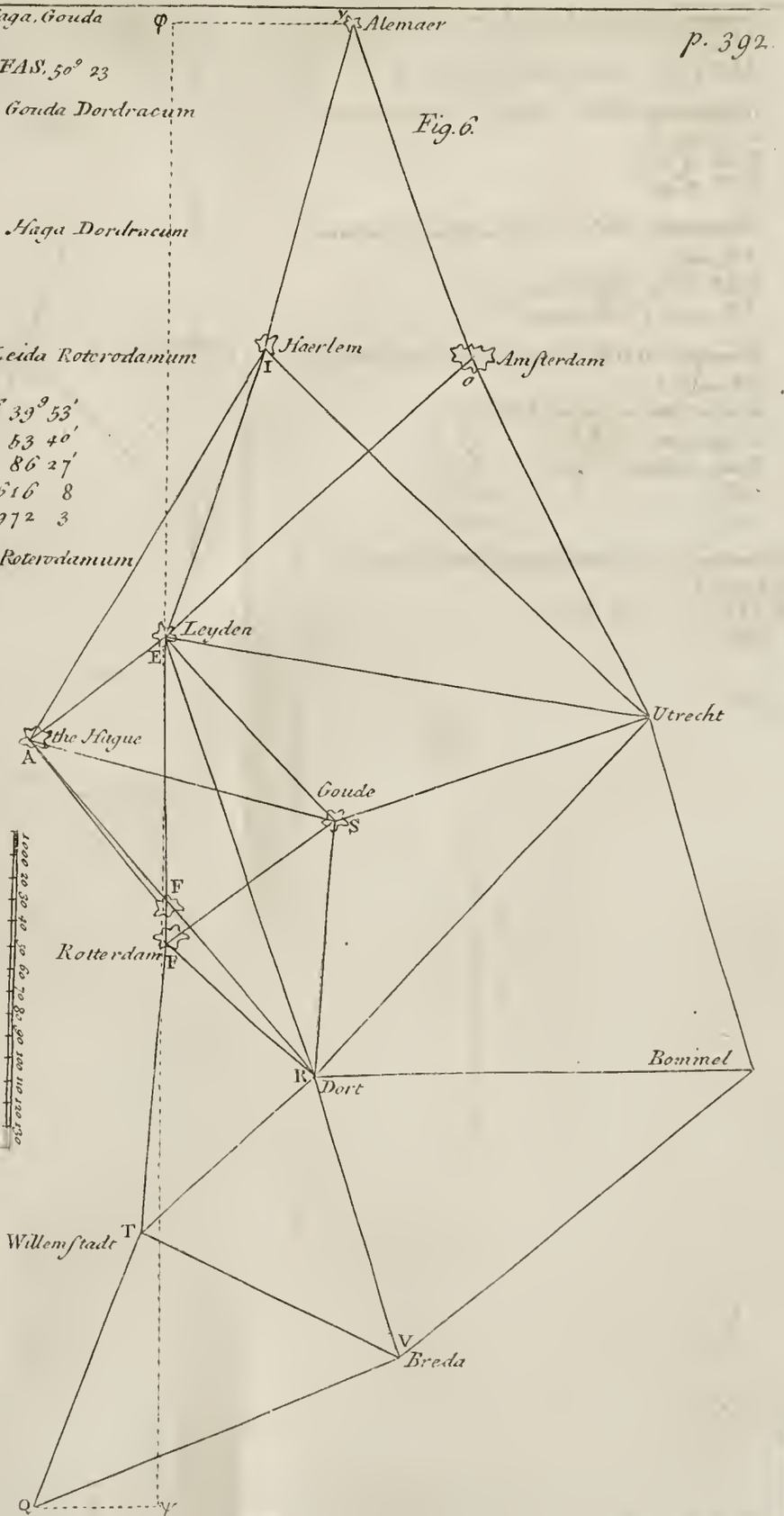

Fig. 6.

C; which I have diſcovered and demonſtrated upon ſeveral ſubjects.

K. The ſpermatic artery.

Fig. 3. A. The extremity of the *cornu uteri.*

B. B. B. The tube.

C. The expanſion of the tube raiſed to ſhew the aperture, the fleſhy fibres, and other neighbouring parts, faſtened to the ovary marked O. O.

D. D. D. D. D. Fleſhy fibres, ſeveral of which are faſtened to the ovary marked O. O.

E. E. Two *ova*, a part of which was on the outſide, the membrane of the ovary and the other part on the inſide, appearing like the point of an acorn out of its cup.

F. An aperture ſhaped like a half creſcent, which has been mentioned ſeveral times.

Fig. 4. Is an ovary uncovered.

A. A. A. A great ſpongy body, of which,

I. I. Is the point.

R. The aperture, which is found ſometimes, and almoſt always at certain ſeaſons.

N. The entrance of a cavity, which leads to the outer part, which is ſeen only by blowing.

P. P. Half the ovary, wherein are ſeen ſeveral eggs, almoſt detached from their cup.

Fig. 5. Is an ovary ſeparated at the place where the veſſels enter, where eggs of different ſizes are ſeen.

T. T. Is an *ovum* emptied of liquor, to ſhew how much the membrane of the ovary is thinned, at the place where the egg muſt go out.

Fig. 6. A. A. A. A. A. A. A part of the *cornu uteri.*

S. S. S. S. S. The hood, which exactly embraces the ovary on all ſides.

O. O. O. O. O. O. The tube, of which the expanſion is hidden by the hood.

Fig.

Fig. 7. ſhews another ovary, which was alſo embraced and hidden by the hood.

A. A. A. A. Part of the *cornu uteri.*

B. B. B. B. The hood opened.

C. C. C. The ovary, upon which are ſeen ſeveral eggs, one of which appeared to be raiſed.

D. The ſpongy body.

I. The place where it was opened.

X. An aperture ſhaped like a half creſcent, of which the edge was raiſed, which paſſes over the egg when it is blown into thro' a pipe.

Fig. VIII. and IX. repreſent an ovary on the outſide and inſide.

Fig. VIII. A. A. The ſpongy body ſeparated from the veſicular membrane or ovary.

B. B. The part of the ovary, where the *ova* were.

C. C. The two apertures ſhaped like a half creſcent.

Fig. IX. A. A. The ſame ovary ſeen on the inſide, in which ſeveral *ova* are diſcovered.

B. B. An egg emptied of the liquor, which ſhews by blowing in with a pipe the neareſt external aperture to the ſpongy body.

VI. *On the circulation of the blood in thoſe fiſhes which have gills, and on their reſpiration, by M.* du Verney *ſenior* *.

It is known by every body, that the heart of all fiſhes, which do not breath air, has but one cavity, and conſequently but one auricle, at the outlet of the veſſel which returns the blood. That of the heart of the carp is applied to the left ſide.

The fleſh of the heart is very thick in propor-

Nov. 29, 1701.

tion

Pl. XV. *Vol. I.*

Fig. 1.

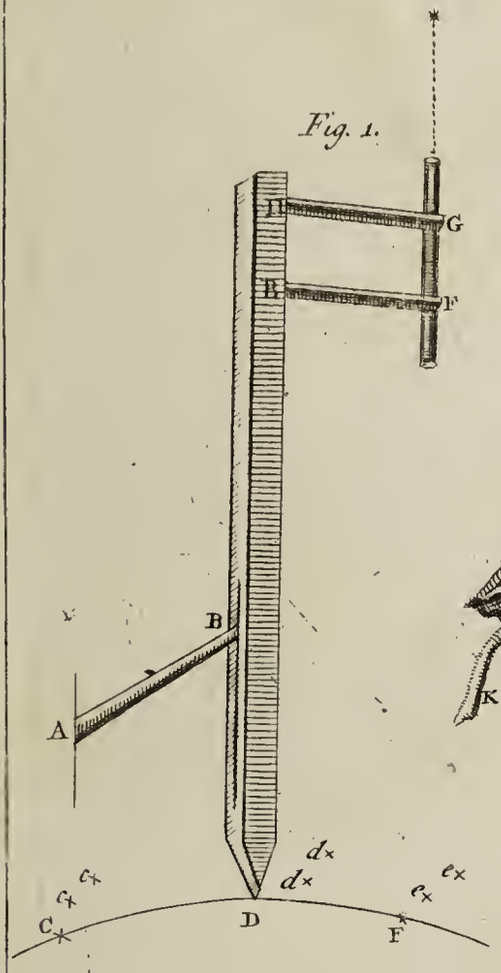

Fig. 2.

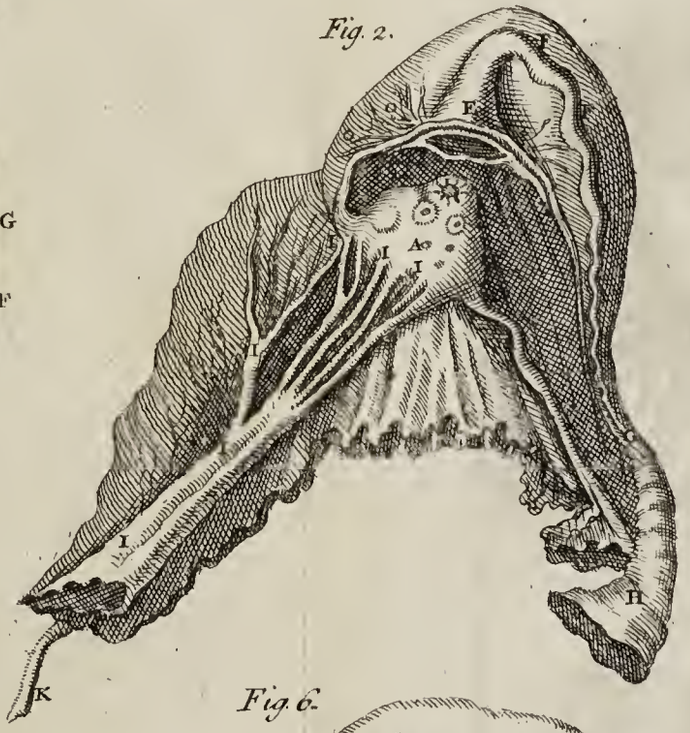

Fig. 3.

Fig. 4.

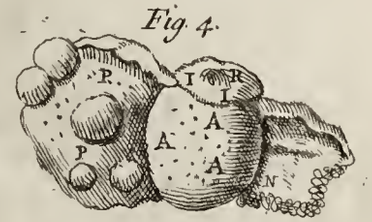

Fig. 5.

Fig. 6.

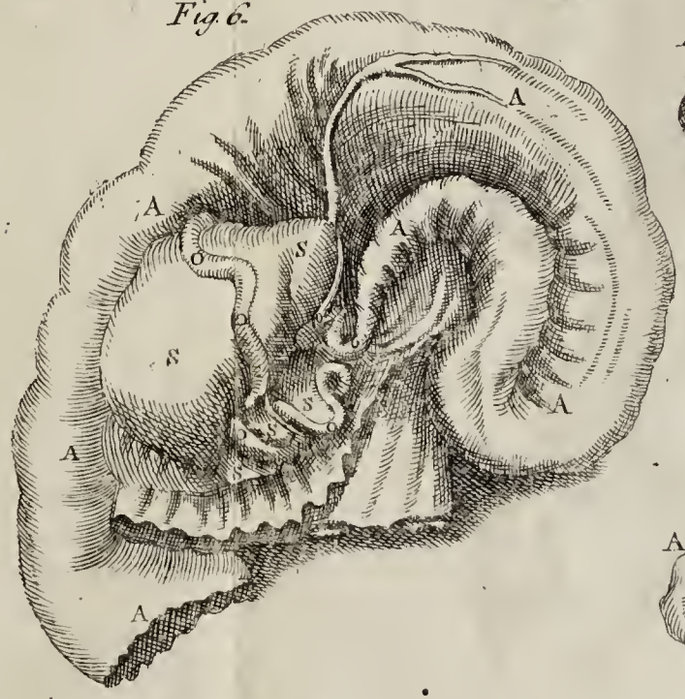

Fig. 7.

Fig. 8.

Fig. 9.

J. Mynde sc.

tion to its bulk, and its fibres are very compact: thus it has need of a very ſtrong action for the circulation, as we ſhall ſee hereafter.

Every one knows what gills are; but every one does not know, that theſe parts ſerve for lungs to fiſhes. Their fabrick is compoſed of 4 ribs on each ſide, which move upon themſelves in opening and ſhutting, and alſo with regard to their upper and lower ſupports, in receding from and approaching towards each other. The convex ſide of each rib is charged on both its edges with two ſorts of *laminæ*, each of which is compoſed of a row of narrow *lamellæ*, ranged and locked againſt each other, which form as it were ſo many beards or fringes, like the vanes of a feather; and theſe fringes are properly called the lungs of the fiſhes.

Here is a very extraordinary, and very ſingular ſituation of the parts. The *thorax*, as well as the lungs, is in the mouth: the ribs bear the lungs, and the animal breaths water.

The extremities of theſe ribs, which are towards the throat, are joined together by ſeveral little bones, which form a ſort of *ſternum*; in ſuch ſort however, that the ribs have a much freer play upon the *ſternum*, and can recede from each other much more eaſily than thoſe of men, and that the *ſternum* can be raiſed and depreſſed. The other extremities, which are towards the baſe of the ſkull, are alſo joined by ſome little bones, which are articulated with this baſe, and can recede from it, or approach to it.

Each rib is compoſed of two pieces, joined by a very ſupple cartilage, which in each of theſe parts is what the hinges are in mechanical works.

The firſt piece is bent into an arch, and its length

length is about a ſixth portion of the circle of which it would be a part.

The ſecond almoſt deſcribes a capital *Roman* S.

The convex part of each rib is hollowed like a gutter, and the veſſels, which I ſhall mention afterwards, run along this gutter.

Each of the *laminæ*, of which the *lamellæ* are compoſed, is of the figure of a ſcythe, and at its origin it has a foot, which reſts only by its extremity on the edge of the rib.

Each of theſe *laminæ* is compoſed of 135 *lamellæ*, thus the 16 contain 8,640 ſurfaces, which I reckon here, becauſe the 2 ſurfaces of each *lamella* are cloathed all over with a very fine membrane, upon which are made almoſt innumerable ramifications of the capillary veſſels of theſe ſorts of lungs.

I have ſhewed the company, that there are 46 muſcles employed in the motions of theſe ribs; 8 of them dilate the interval, and 16 contract it, 6 inlarge the arch of each rib, and 12 contract it, and at the ſame time depreſs the *ſternum*; and the other 4 raiſe it up.

The gills have a large aperture, upon which is placed a cover compoſed of an aſſemblage of ſeveral pieces; it has the uſe of the leather of a pair of bellows, and each cover is formed with ſuch artifice, that, in ſeparating from each other, they bend outwards to augment the capacity of the mouth, whilſt one of their pieces, which plays on a ſort of knee, keeps the apertures of the gills cloſe, and opens them only to give paſſage to the water which the animal has reſpired, which is done at the ſame time that the cover is depreſſed and contracted.

There are 2 muſcles, which ſerve to raiſe this cover,

cover, and 3 which ſerve to depreſs and contract it.

I have juſt ſaid, that the aſſemblage, which compoſes the ſtructure of the covers, makes them capable of bending outwards. I ſhall add 2 other circumſtances. 1. That the part of this cover, which aſſiſts in forming the under part of the throat, is plaited like a fan upon little bony *lamellæ*, to ſerve, as it unfolds, for the dilatation of the throat in the inſpiration of the water. 2. That each cover is cloathed both within and without by a ſkin which adheres very cloſely to it. Theſe 2 ſkins joining together, are prolonged to about 2 or 3 lines beyond the circumference of the cover, and diminiſh continually in thickneſs. This prolongation is much larger under the throat, than towards the top of the head. It is very ſupple, that it may be applied the more exactly to the aperture upon which it reſts, and to keep it ſhut at the firſt moment of the dilatation of the mouth for reſpiration.

The artery, which comes out of the heart, dilates itſelf in ſuch a manner, as to cover the whole *baſis* of it; and then contracting gradually it forms a ſort of cone. At the place where it is thus dilated, it is furniſhed on the inſide with ſeveral fleſhy columns, which may be conſidered as ſo many muſcles, which make of this part of the *aorta* a ſort of ſecond heart, or at leaſt a ſort of ſecond ventricle, which joining its preſſure to that of the heart, doubles the force neceſſary for the diſtribution of the blood, in order to the circulation.

This artery riſing by the interval between the gills, makes over againſt each pair of ribs on each ſide, a great branch, which runs in the

gutter hollowed on the outer surface of each rib, from one extremity of the *lamina* to the other. This is the course of the *aorta* in the animals of this kind. The *aorta*, which in other animals, carries the blood from the centre to the circumference of the whole body, runs in these, no farther than from the heart to the extremity of the gills, where it ends.

This branch furnishes as many twigs, as there are *lamellæ* on each edge of the rib. The great branch ends at the extremity of the rib, as has been said, and the twigs end at the extremity of the *lamellæ*, to which each of them is distributed.

Each twig of an artery, rises along the inner edge of each *lamella* of the 2 *laminæ* placed upon each rib, that is, along both edges of the *lamellæ*, which are towards each other, these 2 twigs inosculate in the middle of their length; and continuing their course, reach as I have said, to the point of each *lamella*. There each twig of the extremity of the artery, finds the mouth of a vein; and these 2 mouths being immediately applied to each other, and making but one canal, notwithstanding the different consistence of the 2 vessels, the vein goes down upon the exterior edge of each *lamella*, and being come to the bottom of the *lamella*, it pours its blood into a great venal vessel, couched near the branch of the artery, in the whole extent of the gutter of the rib. But it is not only by this immediate inosculation of the 2 extremities of the artery and the vein, that the artery discharges itself into the vein, but also by its whole course, in the following manner.

The twig of the artery upon the edge of each *lamella* throws off, thro' its whole course, upon

the

the flat of each *lamella* on both ſides, an infinite number of veſſels; which parting by pairs from this twig, one on one ſide, and the other on the other, each of them on its own ſide goes ſtrait to the vein, which deſcends upon the oppoſite edge of the *lamella*, and inoſculates with it by an immediate contact. It is thus that the blood paſſes in this kind of animals, from their pulmonary arteries into their veins from one end to the other. Their arteries are true arteries, both by their ſtructure, and by their carrying the blood. Their veins are true veins, both by their receiving the blood from the arteries, and by the extreme delicacy of their conſiſtence. Thus far there is nothing but what is in the uſual œconomy. But the ſingularities are, 1. the immediate inoſculation of the arteries with the veins, which is found indeed in the lungs of other animals, eſpecially in thoſe of frogs and tortoiſes, but is not ſo manifeſt as in the gills of fiſhes. 2. The regularity of the diſtribution, which renders this inoſculation more viſible in this kind of animals; for all the branches of arteries, riſing along the *lamellæ* upon the ribs, are as ſtrait, and as equally diſtant from each other, as the *lamellæ*. The tranſverſe capillary twigs, which part from theſe branches at right angles, are equally diſtant from each other; ſo that the direction and the intervals of theſe veſſels, both the upright and the tranſverſe, being as regular, as if they had been drawn by rule and compaſs, we follow them with the eye and with the microſcope. We ſee therefore that the tranſverſe arteries end immediately in the body of the deſcending vein, and each of theſe veins, having received the blood from the tranverſe capillary arteries on each ſide of the

lamella, inoſculates directly with the trunk of the vein couched in the gutter.

It muſt be confeſſed, that this diſtribution is very ſingular: but what follows is more ſo. Theſe trunks of veins, full of arterial blood, coming out on each ſide by their extremity, which is towards the *baſis* of the *cranium*, take the conſiſtence and thickneſs of arteries, and unite themſelves again by pairs on each ſide. That of the firſt rib furniſhes, before its reunion, ſome branches, which diſtribute the blood to the organs of ſenſation, the brain, and neighbouring parts, and by this means performs the functions, which belong to the aſcending *aorta* in quadrupeds, then it joins itſelf again to that of the ſecond rib, and theſe two together make but one trunk, which running along the *baſis* of the *cranium*, receives again on each ſide another branch formed by the reunion of the veins of the third and fourth parts of the ribs, and altogther make but one trunk.

Afterwards this trunk, all the roots of which were veins in the lungs, becoming an artery, by its coat and office, continues its courſe along the *vertebræ*; and diſtributing the arterial blood to all the other parts, performs the function of a deſcending *aorta*, and the arterial blood is by this means equally diſtributed to all the parts, to nouriſh and animate them; and there are roots of veins every where, which take up the reſidue again, and carry it back by ſeveral trunks formed by the union of all theſe roots, to the common reſervoir, which muſt return it to the heart; thus the circulation is finiſhed in theſe animals.

But what increaſes the ſingularity, is that theſe very pulmonary veins coming out of the gutter of the ribs by their extremity towards the throat, preſerve the coat and function of veins, by bring-

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ing back, into the recepticle of all the venal blood, a portion of the arterial blood, which they have received from the pulmonary arteries.

As the motion of the jaws contributes alſo to the reſpiration of fiſhes, it will not be amiſs to obſerve, that the upper one is moveable, that it is compoſed of ſeveral pieces, which are naturally engaged in each other, in ſuch a manner, that they may, by unfolding, dilate and prolong the upper jaw.

All the parts, which ſerve for the reſpiration of the carp, amount to ſo ſurpriſing a number, that it cannot be amiſs to enumerate them in this place.

The bony parts are in number 4386: and there are 69 muſcles.

The arteries of the gills, beſides their eight principal branches, divide into 4320 twigs; and each twig makes on each ſide, on the flat of each *lamella*, an infinite number of tranſverſe capillary arteries, the counting of which will not be difficult, and will much exceed all theſe numbers together.

There are as many nerves as arteries, the ramifications of the firſt exactly following thoſe of the reſt.

The veins, as well as the arteries, beſides their eight principal branches, make 4320 twigs, which are mere tubes, and differ from the twigs of the arteries, in not forming any tranſverſe capillary veſſels.

The blood, which is brought back from all the parts of the body of fiſhes, enters the receptacle, into which all the veins empty themſelves into the auricle, and thence into the heart; which, by its contraction, drives it into the *aorta*, and into all the ramifications, which it makes upon

the

the *lamellæ* of the gills: and as at its origin it is furniſhed with ſeveral very thick fleſhy columns, which contract immediately afterwards; it ſeconds and fortifies by its compreſſion the action of the heart, which is to drive the blood with much more force into the tranſverſe capillary twigs, ſituated on each ſide, upon all the *lamellæ* of the gills.

It has been obſerved, that this artery and its branches run only from the heart to the extremity of the gills. Thus this redoubled ſtroke of a piſton muſt ſuffice to drive the blood, with impetuoſity, into this infinite number of little arteries, ſo ſtrait and ſo regular, where the blood finds no other obſtacle than meer contact; and not the ſhock and the reflections, as in other animals, where the arteries are ramified a thouſand ways, eſpecially in their laſt ſubdiviſions.

So much for the paſſage of the blood into the lungs: it remains for me now to ſpeak of the preparation of it.

I ſuppoſe, that the particles of air, which are in water, as water is in a ſponge, are able to diſengage themſelves from it ſeveral ways. 1. By heat, as we ſee in boiling water. 2. By the weakening of the ſpring of the air, which preſſes the water, in which theſe particles of air are engaged, as we ſee in the air-pump. 3. By the agitation and extreme diviſion of the water, when it has any degree of heat.

We cannot queſtion there being a great deal of air in the bodies of fiſhes, and that this air is very neceſſary for them. Both theſe are ſhewn by the air-pump.

I put a very briſk tench into a veſſel full of water, which was placed under a receiver; and after having given 5 or 6 ſtroaks of the piſton,

we obſerved that this tench was quite covered with an infinite number of little bubbles of air, which got out from between the ſcales; ſo that the whole body ſeemed ſet with pearls. There alſo got out a very great number by the gills, much bigger than thoſe on the ſurface of the body. At laſt ſome came out by the mouth, but in leſs quantity. Beginning to pump anew two or three times ſucceſſively, we obſerved that the fiſh was agitated and tormented in an extraordinary manner, and that it breathed more frequently. After having ſpent a full quarter of an hour in this condition, it grew languid, the whole body and even the gills having no longer any ſenſible motion. Then taking the veſſel from under the receiver, we threw the fiſh into common water, where it began to breath and ſwim, but faintly, and it was a long time in returning to its natural ſtate.

I made the ſame experiment on a carp. I put it into the ſame machine, and having pumped the air 3 or 4 times, as we had done from the tench, the fiſh began at firſt to be agitated; the whole ſurface of the body became pearled, there came out of the mouth and gills an infinite number of very large bubbles of air, and the region of the air-bladder ſwelled very much.

Tho' this carp was bigger than the tench, the beating of the gills ceaſed ſooner. When we began to pump again, the gills began to beat again, but only for a little while, and very faintly. At laſt it continued without any motion; and the region of the bladder became ſo ſwoln and ſo ſtretched, that the row began to come out at the *anus*. This laſted about $\frac{3}{4}$ of an hour, when it died, being grown very flat. Having opened it we found the bladder burſt.

We

We alſo found by experiment, that a fiſh being put into water purged of air, cannot live there long.

Beſides theſe experiments, which may be made in the air-pump, here are others, which prove alſo that the air, which is mixed in the water, has the principal ſhare in the reſpiration of fiſhes.

If you incloſe ſome fiſhes in a glaſs-veſſel full of water, they live for ſome time, provided the water is renewed: but if you cover the veſſel, and ſtop it, ſo that the air cannot get in, the fiſhes will be ſtifled. This proves that the water ſerves for their reſpiration, only ſo far as it has liberty to be impregnated with air.

Put ſeveral fiſhes into a veſſel, which is not entirely filled with water, if you cloſe it, theſe fiſhes, which before ſwam at full liberty, and were briſk, will be agitated, and preſs towards the upper part, to reſpire that portion of the water, which is neareſt to the air.

We obſerve alſo, that when the ſurface of the ponds is frozen, the fiſhes die ſooner or later, according as the pond is of greater or leſs extent and depth; and we obſerve, that when the ice is broken any where, the fiſhes preſent themſelves haſtily to breathe this water impregnated with new air. Theſe experiments manifeſtly prove the neceſſity of air for the reſpiration of fiſhes. Let us ſee now what paſſes in the time of this reſpiration.

The mouth opens, and the lips advance, and thereby the cavity of the mouth is prolonged, the throat ſwells, the coverings of the gills, which have the ſame motion with the leathers of a pair of bellows, ſeparating from each other, arch outwards by their middle only, whilſt one of their pieces, which plays upon a ſort of knee, keeps the

the apertures of the gills ſhut, continually raiſing themſelves a little, without permitting the water to enter; becauſe the little ſkin, which edges each covering, cloſes the aperture of the gills exactly.

All this increaſes and inlarges every way the capacity of the mouth, and determines the water to enter into its cavity, juſt as the air enters by the mouth and noſtrils, into the *trachea* and lungs, by the dilatation of the breaſt. At the ſame time the ribs of the gills open, by ſeparating from each other; their arch is enlarged, the *ſternum* is withdrawn from the palate; and ſo every thing conſpires to make the water enter the mouth in greater quantity. Thus the inſpiration of fiſhes is performed. Afterwards the mouth ſhuts; the lips, which before were prolonged, contract themſelves, eſpecially the upper one, which folds like a fan, the lower lip ſticks to the upper one by means of a little ſkin in form of a creſcent, which falls down like a curtain, and hinders the water from getting out. The cover is flatted upon the aperture of the gills. At the ſame time the ribs cloſe againſt one another, their arch is contracted, and the *ſternum* ſinks upon the palate.

All this contributes to compreſs the water which is entered into the mouth. It then preſents itſelf to get out by all the intervals of the ribs, and by thoſe of their *lamellæ*, and it paſſes like ſo many wires; and by this motion the membranous border of the covers is raiſed, and the water being preſſed eſcapes by this aperture. Thus the expiration is performed in fiſhes. We here ſee, that the water goes in at the mouth, and out at the gills by a ſort of circulation, quite contrary to what happens in quadrupeds, in which the air goes

in and out alternately by the ſame aperture of the *trachea*.

This is all that concerns the motions of the reſpiration of fiſhes. Let us now purſue the courſe of the blood in the gills, and ſee what preparation it there receives.

The blood, which comes out of the heart of the carp, ſpreads itſelf in ſuch a manner upon all the *lamellæ*, of which the gills are compoſed, that a very ſmall quantity of blood preſents it ſelf to the water under a very great ſurface; that by this means each of its parts may more eaſily and in leſs time be penetrated by the particles of air, which are diſengaged from the water, by the extreme diviſion that it ſuffers between theſe *lamellæ*. It is for this that it was neceſſary not only that each *lamina* ſhonld have ſo great a number of them, but alſo that all their ſurfaces ſhould be covered with tranſverſe capillary branches of the *aorta*.

We obſerve in ſome meaſure the ſame mechaniſm in the lungs of other animals; for they are formed of a prodigious number of ſmall membranous veſicles, which ſupply the place of *lamellæ*, they are interwoven with an infinite number of ſmall veſſels, which make the blood ſpread itſelf in the ſubſtance of the lungs in ſuch a manner, as to preſent itſelf to the air under a very great ſurface.

But the number of theſe veſſels in the veſicles of the lungs does not come near the number of thoſe of the *lamellæ*. It is alſo more difficult to draw air from water, than to breathe pure air, ſuch as enters into the veſicular lungs.

If we conſider the extraordinary concuſſion and diviſion, which the parts of air ſuffer in the time of expiration, we ſhall be apt to believe, that it

is then that the air enters into the capillary veſſels of the gills. It is therefore probable, that the ſame thing happens in the lungs of other animals; for as the air muſt have ſome force to inſinuate itſelf into the veſſels, it does not appear that it can enter them in the time of inſpiration, that is, when it enters naturally into the lungs. On the contrary, when it is repelled by the expiration, it endeavours to eſcape on all ſides; and forcing all the obſtacles that it meets, it paſſes through the thin and fine membranes, which compoſe the veſſels, whilſt the greateſt part of this air goes out again by the *trachea*.

The difficulty, with which theſe ſmall parts of air paſs through the pores of theſe veſſels, compreſſes their ſpring; whence it follows, that when they are entered, this ſpring muſt diſcharge with impetuoſity againſt the particles of the blood, which are then depreſſed, agitated, and pounded with violence; which makes them ſhock together every way; and it is by this that they acquire a new motion of liquidity and heat.

If this is true in the animals that breath air, it muſt be ſtill more true in the animals that breath water; becauſe here the air is otherwiſe compreſſed, than the free air is which they firſt reſpire; ſo that the great ſeparation of theſe particles of air ſo compreſſed, muſt in ſome meaſure ſupply the leſs quantity of air, which enters into the veſſels of the gills.

When we conſider that the blood of the veins of the gills is of a more vermillion red, than that of the *aorta*, we eaſily judge, that it is there charged with ſome particles of air. We obſerve in other animals the ſame difference between the blood of the pulmonary artery, which is always

of a dark red, and that of the pulmonary vein, which is always of a very bright red.

The blood thus impregnated with particles of air, and thereby become truly arterial, enters into the veins of the gills; and theſe veins coming out of the gutter of the ribs by the extremity, which is towards the *baſis* of the *cranium*, take the conſiſtence of arteries, and diſtribute this blood to all the parts. It is afterwards taken up again by the veins which carry it to the heart.

It muſt not be forgot, that the artery which proceeds from the heart, has a pulſation, whereas the veſſels, which perform the function of the *aorta*, have none at all, or at leaſt none that is ſenſible. 1. Becauſe they have no immediate communication with the heart. 2. Becauſe the blood paſſes from a ſmall tube into a great one. But it muſt alſo be conſidered, that the throwing of the blood is not neceſſary for the nutrition of the parts, for which it is ſufficient that the blood flows with a gentle courſe; as it is alſo neceſſary, that it ſhould flow otherwiſe for its diſtribution and circulation, eſpecially in thoſe animals, where it is much ſlower, which perſpire but little, and can live a long time without any nouriſhment.

It is eaſy to judge by all that has been juſt ſaid, that the ſituation and conformation of the lungs and their commerce with the heart, are very different in the different ſpecies of animals, which was not unknown to *Malpighi*.

In the *fœtus*, there are particular ducts, which have ſo near a communication with the ventricles of the heart, and the head of the veſſels of the lungs, that they make almoſt all the nutritious juices of the mother paſs immediately into the *aorta*, which diſtributes them to all the reſt

of

of the body: whereas, after the birth, all the blood of the veins enters into the right ventricle, which drives it immediately into the lungs; from whence, after it has in a long circuit been impregnated with particles of air, it passes into the left ventricle, which spreads it immediately by the *aorta* into all the parts.

In tortoises, frogs, and other animals analogous to them, $\frac{1}{3}$ of the blood passes through the lungs at each circulation, and there receives all the preparations necessary for the functions of life. This blood, which returns from the lungs, mixes afterwards with that of the veins in the cavity of the heart, where this last being impregnated with the active parts of the air, which the first received in the lungs, is afterwards distributed by the *aorta* to the whole body.

In fishes, all the blood, which comes out of the heart, passes through the lungs, where being also impregnated with the active parts of the air, it afterwards distributes itself through the whole body, and so far this circulation is conformable to that of man. Fishes however have but one single ventricle: but this singular circulation is occasioned by the *aorta*, performing the office of the pulmonary artery, and by the pulmonary veins becoming arteries and performing the office of the *aorta*.

In insects, the *tracheæ*, which serve them for lungs, are spread through all the parts, where they are ramified after the manner of the *bronchia* in the vesicular lungs; so that whereas in other animals the air borrowed from the *bronchia* is distributed into all the parts by the arteries, it is here immediately distributed into the juices which are actually in each part.

The reason of so surprising a distribution comes from

from the nature of the liquors contained in the tubes of theſe animals, which, that they may be extremely glutinous and viſcid, and conſequently very fit to be connected together, and ſtuck to the ſurface of their veſſels, ought to be impregnated, in their whole courſe, with the active parts of air, to facilitate their circulation, and render them fit for nutrition.

We ſee by this enumeration, that the functions of the lungs have not always a cloſe connection with thoſe of the heart, and that each of theſe parts have very different uſes, with regard to the blood.

The heart is only for the motion, which we call circulation. The lungs favour it by the introduction of the particles of air, and alſo by the impulſe of the water into the animals in queſtion. But its principal office is to impregnate the blood with air, and thereby to render it capable of carrying the aliment, life, and heat all over. It is for this reaſon, that we have juſt ſhewn, 1. That in all animals, except inſects, the blood never paſſes from the heart into the *aorta*, without having paſſed through the lungs, even in the *fœtus*, in the manner that we have explained. 2. That in moſt it muſt neceſſarily paſs thither entire, as in men, quadrupeds, birds, and fiſhes. 3. Or that it paſſes thither in part, as in tortoiſes, frogs, *&c.* And it is neceſſary, that at leaſt $\frac{1}{3}$ of the blood ſhould paſs through the lungs of theſe animals, to be vivified as much as their functions require.

Laſtly, we have ſhewn, that if in inſects there are no lungs for the blood to paſs through, it is becauſe the air is neceſſarily mixed in all their parts with the nutritious juices; ſo that by this mechaniſm each part ſerves for lungs to itſelf.

A

TABLE

OF THE

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AN

AN ABRIDGMENT OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS, in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1702.

I. *On the effects of the elaſticity of the air in gunpowder and thunder, tranſlated by Mr.* Chambers.

THE air, which, till of late, appeared a fluid almoſt void of action, is now found one of the moſt violent and univerſal agents in all nature. The force, for inſtance, of gunpowder, how ſurpriſing ſoever, is only the force of the air. There is air incloſed, or rather crowded and impriſoned, in each grain of the powder; and there is likewiſe air in all the intervals between the grains, and when the powder takes fire, the ſprings of all theſe little portions of air open and unbend themſelves all at once. Theſe ſprings are the only cauſe of ſuch prodigious effects, the powder only ſerving to light a fire to put the air in action; after which 'tis the air alone that is to do all.

Hence M. *de la Hire* has ventured to aſcribe all the *phænomena* of gunpowder to the properties of elaſticity, the principal whereof, or at leaſt thoſe of moſt importance in his enquiry, are as follow.

A ſpring, for inſtance, a bended leaf, or *lamella* of ſteel, tends to unbend itſelf on the two

oppoſite ſides with equal violence, it requires as certain reſiſtance to enable it to exerciſe its whole force, and it acts ſo much the leſs, as the body it acts againſt yields or gives way more eaſily. A ſpring, in fine, has a greater effect on one ſide, when it meets with a reſiſtance on the oppoſite one.

On theſe ſuppoſitions, M. *de la Hire* firſt conſiders all the ſprings of the air put in action by the fire which lights the powder in the barrel of a gun: ſome philoſophers have imagined, that when lighted ſucceſſively, its effort is greater in the part where it was firſt kindled, by reaſon its violence is here augmented by that which kindles afterwards: but this muſt be a miſtake; for according to the remark of M. *de la Hire*, a ſpring reſting upon another equal ſpring, and reſiſted by it, has all the force it is capable of, nor will receive any addition by any number of other ſprings ſucceeding each other, either to ſuſtain it, or to ſuſtain thoſe it bears upon. On the contrary, the force of the firſt may perhaps be diminiſhed, while the others are put in motion; and if during this ſpace of time, the body againſt which they are to act begins to give way, their action of conſequence will be ſo much the weaker.

'Tis better therefore that the ſprings all unbend together, even though one would only have them act in the place where the powder firſt begins to take fire; 'tis evident beſides, that if the powder kindle all at once, a greater heat will ariſe, and conſequently the ſprings be put in a greater tenſion; and that as they will hereby ſuſtain each other at the ſame time, they will be capable of the greater effort towards every ſide, all we have to be apprehenſive of is the cannon's burſting, by a too haſty and ſudden inflamation of the whole powder;

powder; on which account it has been found proper to have it a little diminiſhed.

The cannon being ſuppoſed thick enough to reſiſt all the impulſe directed from the *axis* of the barrel towards its circumference, there only remains that which is in the direction of the breech and mouth, the ſprings drive equally in thoſe two contrary ways, and hence it happens, that the cannon rebounds or flies backwards, while the ball iſſues the other way.

The force therefore, which cauſes the rebound, is the ſame as that which cauſes the motion of the ball, whence then does it proceed, that the motion of the ball is of ſo great an extent, and that of the rebound ſo little? The reaſon is, that the cannon finds much more oppoſition to its moving backwards, than the ball to its moving forwards; and as the two effects ariſe from an equal power, the way paſſed over by the ball, is ſo much greater, than that paſſed by the cannon, as the difficulty of its moving backwards ſurpaſſes that of the ball's moving forwards.

Hence there muſt be a great reſiſtance made to the rebound of the cannon. This reſiſtance we ſeem to have from the friction of ſo heavy a machine, as a cannon with its carriage againſt the ground; but this is not all, for the reſiſtance of a motion is ſo much the greater, as this motion is quicker; and when its quickneſs is ſuch, as that the reſiſting power has no time to give way in this caſe, a body very weak of itſelf may do the office of any movable obſtacle. Thus it is, that the air and water, when ſtruck ſo haſtily, and with ſuch nimbleneſs, that they have not time to recede, become fixed points; one of them for the flight of birds, and the other for the action of oars; ſo a ſtaff ſuſpended at the two ends, by

two ſlender threads, may be ſtruck in the middle with ſo quick a motion, as to break it in two without injuring the threads it was ſuſtained by; for the fibres of ſuch threads require a certain time to be lengthened and extended, ſo as to ſeparate, which time is not here allowed them; beſides, that the air not having been able to eſcape quick enough from behind the ſtaff, ſuſtained it as it were, and thus expoſed it to receive the whole impreſſion of the ſtroke, the extreme velocity therefore, or as I would rather call it, the extreme ſuddenneſs of the motion, which the powder gives the cannon, muſt augment the reſiſtance made to its rebounding, either by the ground, or even by the air, if a cannon were ſuſpended, experience ſhews us, that the rebound would be very great.

A rocket, whoſe conſtruction I here ſuppoſe known, is only a little light cannon, which by the impulſe of the lighted matter it contains, rebounds upwards in the direction of its breech, and this with the ſame velocity, as that wherewith the combuſtible matter ſtreams out at its mouth, which looks downwards in effect; this rebound is the riſe of the rocket.

When the rocket is charged with all its matter, if its centre of gravity were above the centre of its figure, with regard to the cloſe end, which is what goes foremoſt, it would follow from ſome reaſons, laid down in the hiſtory of 1700 *, that as ſoon as the rocket began to riſe, it would deſcribe a ſemicircle in the air, till it became upſide down, and after this would fall back again, by reaſon the cloſe end which makes the rebound, would be turned towards the earth; now it being impoſſible in the practice to determine the preciſe place of the centre of gravity and its

* Pag. 191. of this abridgment.

its poſition, with regard to its centre of magnitude, recourſe is had to a ſhorter and eaſier expedient, a ſtick is tied to one end of the rocket, whoſe weight is ſuch, as that the centre of gravity of the rocket and ſtick taken together, falls a little below the aperture of the rocket ; if this centre be below that aperture when the rocket is intire, it will be more below it ſtill when it riſes, ſince it is continually diſcharging its combuſtible matters, and thus becomes lighter and lighter. This centre therefore is always deſcending in proportion as the rocket riſes ; and thus makes it preſerve a direct motion.

M. *de la Hire* confeſſes to the glory of that vague and inaccurate experience, which produced the arts, that he believes the moſt ſubtil ſpeculation cannot add any thing to the conſtruction of rockets ; only he obſerves, that the ſtick being faſtened to one of the ſides, the centre of gravity of the whole cannot be in the axis of the rocket, and conſequently that it can never riſe quite vertically; but that in order to give it this direction, it would be better to faſten two ſticks to the two ſides, both of them only weighing the ſame as one ſhould have done.

'Tis eaſy to apply the principles which obtain in cannons and rockets to petards. To augment the effect of a petard againſt a gate or wall, it is faſtened to, M. *de la Hire* propoſes, that it be rendered perfectly firm and immovable on the oppoſite ſide ; this will prevent its rebounding, and redouble its violence on that ſide it is to act on.

Thunder itſelf is only a kind of lighted gunpowder, and men may without preſumption boaſt they have imitated it. 'Tis a mixture of ſulphur and ſalt petre, or other matters, near akin to them, and the air dilated, and its ſprings put

put in action by the inflammation thereof, produces the principal *phænomena* of thunder.

If this air, when it dilates and unbends, meet with nothing to reſiſt it, we find lightening produced, but hear no noiſe; if it meet with clouds, which oppoſe its motion, there ariſes a friction or colliſion of the air, which occaſions the noiſe, and this noiſe is ſo much the greater, as theſe clouds being formed of little particles of ice, are leſs diſpoſed to receive motion from an inflamed air, when the fire produced by thunder moves with ſo much violence, as to compreſs and ſtrain the ſprings of the groſs air around it, this air becomes capable hereby of reſiſting and driving it back again, which happening ſeveral times together, makes the lightning appear like waving darts of fire.

The air neareſt the earth being the groſſeſt, muſt make the moſt reſiſtance to the motion of thunder, that is, muſt tend moſt to make it reaſcend; and hence the flame muſt be frequently repelled towards the place it aroſe from, and thus ſpend itſelf without effect.

We ſometimes find water ſpout through the adjutage of a fountain, three or four times higher than the height of the reſervoir can occaſion, tho' it quickly returns again to the regular height; if it be enquired how it came to exceed it, M. *de la Hire* attributes it to the air incloſed in the pipe, which having been compreſſed by the water continually deſcending, unbended itſelf againſt that which was aſcending, and gave it this momentary velocity; ſo he alſo imagines, that the violence of thunder may ſometimes be increaſed by the air, which after a ſtrong compreſſion, occaſioned by the fire of the thunder, itſelf reſumes its natural extenſion: were one to purſue all the

effects

effects of the air, it would be found, as it were, the only ſoul of the world, if by world we mean that which we inhabit, and which ſurrounds us neareſt.

II. *On the cauſe of refraction, tranſlated by Mr.* Chambers.

The diſpute about refraction between Meſſ. *Deſcartes* and *Fermat* is famous; they had each of them their way of demonſtrating, that an oblique ray paſſing out of air into water, muſt be refracted towards the perpendicular. But in *Deſcartes*'s demonſtration, it was ſuppoſed, that the rays penetrate water more eaſily than air. Whereas it followed from that of *Fermat*, that the rays penetrate air more eaſily than water; this was the chief point in diſpute between thoſe two great men, and it afterwards divided the otherphiloſophers.

M. *Carré* has eſpouſed *Deſcartes*'s ſide, aſſerting, that the air makes more oppoſition to the paſſage of light than water does, notwithſtanding that it receives a much greater quantity thereof, and reflects it leſs; for thoſe two things may be eaſily ſeparated. According to this author, air is the only body penetrable by light, all other bodies are ſolid with reſpect thereto, and reflect it; and when light paſſes through water, or glaſs, it only paſſes through the air contained in their pores, the proper particles of the glaſs or water reflecting it back again; and hence that multitude of reflections in tranſparent bodies, the particles of that ſpacious fluid, which we properly call air, have a liberty of moving beyond all compariſon greater than thoſe of air incloſed and impriſoned in water or glaſs, now the extreme mobi-

mobility of the particles of free or open air, and their continual agitation every way, proves a great impediment to the motion of a ray of light, disturbing, interrupting, and consequently weakning and diminishing its velocity; whereas the air inclosed in transparent bodies, being less movable, proves less detrimental to the motion of the rays; and hence it follows, that of all bodies penetrable by light, free air is the most difficultly penetrable, and that all other give it the easier passage, as they contain less air and are more dense.

Accordingly glass, which contains less air than water, is more favourable to the passage of light, and refracts it more; so boiling water, which has emitted a great quantity of air, occasions a greater refraction than cold water, and the refraction of oil is but little different from that of glass, by reason it only contains little air, as we find by putting it in *vacuo*; 'tis true spirit of wine in those circumstances bubbles extremely, and consequently seems to contain a deal of air, yet its refraction is equal to that of oil; but this ebullition is of no continuance, and the air which arises from it all at once is but small in quantity.

To establish this system of M. *Carré*, a greater number of experiments should be made upon the magnitude of refraction, in respect of the weight and the quantity of air contained in fluids, and the density of solid bodies which are transparent. In the mean time it is some prejudice in behalf of this opinion, that a musket-ball shot off lightly upon the water, seems to be refracted from the perpendicular; now 'tis certain, that water being more difficult to divide, makes more resistance to the motion of the ball than air does; if it likewise made more resistances to the motion of a

ray,

ray, this too would be refracted from the perpendicular, whereas it is really refracted towards it.

III. *Of the tarantula, and the cure of its bite by muſick, tranſlated by Mr.* Chambers.

M. *Geoffroy*, being returned from a journey to *Italy*, has ſhewn the academy ſome dead tarantulas he brought with him. The tarantula is a large ſpider, with eight eyes and as many legs; but what is moſt ſingular in it, are two horns or trunks, which it is continually moving, eſpecially when in ſearch of food; whence M. *Geoffroy* conjectures them to be movable noſtrils.

The tarantula is not only found about *Tarentum*, from whence its name is taken, and throughout *Apulea*, but alſo in ſeveral other parts of *Italy*, and in the iſland of *Corſica*; only thoſe of *Apulea* are the moſt dangerous, tho' none are much ſo but thoſe on the plains, where the air is conſiderably hotter than on the mountains; and ſome even aſſert, that the animal is never venemous, except in coupling time.

Soon after a perſon has been bitten by a tarantula, there enſues a very acute pain in the part, and ſome hours after a numbneſs; then he falls into a profound ſadneſs, reſpiration grows difficult, the pulſe weakens, the ſight becomes dim, and the eyes look wild; at length all ſenſe and motion are loſt, and the patient dies unleſs he be relieved.

All the help phyſick affords conſiſts in a few operations on the wound, with the uſe of cordials and ſudorifficks; but another much ſurer and more efficacious remedy, which reaſoning and art would never have attained to, as being accidentally diſcovered, *viz.* muſick.

When the perſon bitten is left without ſenſe and motion, a muſician tries different tunes, till he meets with that whoſe notes and modulation ſuit the patient; upon which a ſlight motion or tremor appears in his body, then his fingers begin to move in cadence, then his arms, legs, and by degrees his whole body, till at laſt he riſes on his feet, and falls a dancing, in which he continues, ſtill increaſing both in activity and ſtrength. Some will even dance ſix hours together without reſting; after this the perſon is put to bed, and when he is judged ſufficiently recovered from his firſt dance, they draw him out of bed by the ſame tune for a new dance. This exerciſe holds ſeveral days, ſix or ſeven at moſt, till ſuch time as the patient finds himſelf fatigued, and unable to dance any longer, which is the indication of his cure; for ſo long as the poiſon acts on him, he would dance if they would let him, without any diſcontinuance, till his ſtrength being utterly exhauſted, he ſhould die on the ſpot. When the patient begins to find himſelf a weary, he comes a little to his ſenſes, and recovers as from a profound ſleep, without remembering any thing of what paſt during his acceſs, not even his dancing.

The patient thus recovered from his firſt acceſs, is ſometimes intirely cured; if he be not, there remains a black melancholly and alienation of mind upon him; he ſhuns the ſight of man, and ſeeks for water; and if he be not carefully watched, throws himſelf into the firſt river that falls in his way. To the other ſymptoms of this diſeaſe muſt be added an averſion for black and blue; and on the contrary, an affection for white, red, and green.

If

If the perſon do not die, the fit returns at the year's end, much about the time when he was bitten; ſo that he muſt go to dancing again; ſome have had theſe periodical returns for 20 or 30 years running.

Each patient has his peculiar and ſpecifick tune; but all the airs or tunes in general are of very briſk meaſure.

Thus much is atteſted by perſons of the utmoſt veracity; and was alſo confirmed to the academy, not only by what informations M. *Geoffroy* could procure in *Italy*, but alſo by the letters of a Jeſuit of *Toulon* to F. *Gouye*, who related, that he had ſeen an *Italian* ſoldier bitten by a tarantula danced ſeveral days together.

To facts ſo very extraordinary, 'tis no wonder if a few fables be added; ſuch, for inſtance, as this, that the patient is only affected ſo long as the tarantula that bit him is alive; and that the tarantula itſelf dances to the ſame tune.

'Tis not unreaſonable to ſuppoſe with M. *Geoffroy*, that the poiſon of the tarantula may occaſion an extraordinary tenſion of the nerves, much greater than is ſuited to their reſpective functions, and hence the loſs of memory and motion; but at the ſame time, this tenſion being equal to that of ſome ſtrings of an inſtrument, puts the nerves in motion to a certain tone, and obliges them to vibrate when ſtruck by the undulations peculiar to that tone; and hence the cure by muſick; for motion being thus reſtored to the nerves, by a proper mode, the ſpirits are recalled, which before had almoſt intirely abandoned them.

It may perhaps be added, with ſome probability, and pretty much on the ſame principles, if the patient's averſion for certain colours ariſes

from this, that the tenſion of his nerves, even out of the acceſs, being ſtill different from its natural ſtate, the vibrations thoſe colours occaſion in the fibres of the brain, are too contrary to their diſpoſition, and make a kind of diſſonance therein, which is pain.

IV. *Of a very valuable loadſtone.*

M. *Carré* read a letter written from *Holland*, wherein mention was made of a loadſtone, which weighs 11 ounces, and lifts 28 pounds of iron, that is above 40 times its own weight. They aſk 5000 livres for it.

V. *Of a pyramid of ſalt formed in a cryſtalliſation.*

M. *Homberg* ſhewed a little pyramid of ſalt, which had formed itſelf in a cryſtalliſation. It was not very high in proportion to its baſe; it was hollow within, and in forming itſelf, had its baſe turned upwards. M. *Homberg* explained the fact thus. At firſt, there was formed on the ſurface of the ſalted water a little cube of ſalt, which is the figure naturally affected by ſalt. This cube, tho' heavier than the ſalted water, did not ſink, any more than a needle would do, if laid on gently, and for the ſame reaſon; for when a needle is thus laid on the water, there is a little hollow formed about it, filled only with air, where it is, as it were, in a little boat, becauſe the bulk of the little hollow, and of the needle together, is lighter than a like bulk of water. There was formed a like hollow about the cube of ſalt, which plunged a little in the water without ſinking; ſo that its upper ſurface being not ſo high

as

as that of the water, it continued dry. Along the four ſides of this dry ſurface, other cubes of ſalt were cryſtalliſed, which began by forming a little hollow ſquare, of which the firſt cube formed the baſe. All theſe little cubes together, being heavier than the firſt ſingle one, and being encompaſſed with leſs air in proportion, becauſe they joined the firſt by their inner ſides, plunged a little more in the water, that is, to the upper ſurface of the little cubes which touched the firſt. About theſe, other cubes were cryſtalliſed, which plunged ſtill deeper in the water. This continuing for ſome time, as the hollow ſquare grew bigger, ſo it plunged more and more, and formed the inverted pyramid, which growing at laſt too heavy, precipitated itſelf down to the bottom of the water, where it left increaſing.

VI. *Of a woman delivered of four children at a birth.*

M. *Lemery* has related, that on the 19th of *June*, a young woman of *Lyons*, aged 23, at her firſt lying in, at the end of 7 months, was delivered of 3 boys and 1 girl, all of them 14 inches and ſix lines in length, and they lived long enough to be baptiſed.

VII. *Of the tides on the coaſt of* Bretagne *and* Normandy.

M. *Carré* had a queſtion ſent him from *Bretagne*, why on the N. coaſt of that province the tides continually increaſe from *Breſt* to *St. Malo*, where they are ſo high at the new and full moons, as to riſe 60 and 80 feet; and why from *St. Malo* they

they continually diminiſh along the coaſts of *Normandy?*

M. *Carré* anſwered this queſtion by the figure only of the coaſts and ſtraits. The tide, which from the great extent of the *Atlantic* ocean, ſpreads itſelf on the N. coaſt of *Bretagne*, meets at the ſame time the outlet of *la Manche*, which is a much narrower ſpace than that from which it comes. It muſt ſwell therefore at the entrance of this channel, and take in height what the channel wants in breadth, to contain the water that it brings. The channel is afterwards more contracted, and conſequently the water riſes ſtill more. The town of *St. Malo* is ſituated in a ſort of returning angle, formed by the coaſt of *Bretagne* and *Normandy*, the tide is obliged to take the ſame direction with the N. coaſt of *Bretagne*, that is, a S. W. direction; having this courſe, it goes directly againſt the *Engliſh* coaſt of *Cornwall*; whence it is ſtrongly beat back exactly to the corner where *St. Malo* is. There the waters being retained, and in a manner ſhut up, muſt neceſſarily riſe. But beyond *St. Malo* the tide muſt find more liberty in its courſe along the coaſts of *Normandy*.

VIII. *Of the making of allum.*

M. *Geoffroy* informed himſelf exactly in *Italy* of the manner of making roch allum at *Civita Vecchia*. There are near that city ſome quarries of a greyiſh or reddiſh ſtone, pretty hard, and like the *Travertin*. They calcine it in ovens, and then diſſolve the lime in water over a great fire, the water gets all the ſalt out of it, and this ſalt is allum; there ſeparates an uſeleſs earth from it, and at laſt they leave this water to reſt impregnated

pregnated with a ſalt, which for ſome days cryſtalliſes of itſelf, as tartar does about the caſks, and makes what is called roch allum. This is only a general account of the operation; but M. *Geoffroy* relates all the particulars.

Allum is made alſo at *Solfatara* near *Puzzoli*, in the kingdom of *Naples*. *Solfatara* was formerly a burning mountain, of which there now remains nothing but ruins, and a crown or cincture of white, yellowiſh, dry, half burnt and calcined rocks, from which there comes out a very thick ſmoak in ſeveral places. The tradition of the country is, that the earth between theſe rocks, which made the top of the mountain, ſunk down to a certain height. They go up over burning rocks to go down again into a little low plain, which muſt have been the top. It is almoſt oval, 1246 feet long in its greateſt extent, and 1000 feet broad. The ſoil of this plain is a yellow and white ſubſtance, very ſalt, and ſo hot in ſome places, that one cannot hold ones hand long upon it. In ſummer, there ariſes on the ſurface of this earth a ſalt flower or duſt, which they need only ſweep, and ſhove into ſome pits of water, which are at the bottom of the plain; after which, to evaporate this water, being well loaded with ſalt and purified from the earth, there needs no other fire than that which burns under the mountain; the water is put into cauldrons ſunk into the ground, without any other trouble. This allum is not ſo much eſteemed as that of *Civita Vecchia*. They make ſulphur alſo at *Solfatara*, and thence the place has obtained its name.

M. *Geoffroy*, to render his hiſtory of allum more complete, has added the manner of making it in *England*, in *Yorkſhire* and *Lancaſhire*, and alſo in *Sweden*.

It appears by all the preparations of allum, that the ſame mine which affords it, does alſo, or may at leaſt afford ſulphur, nitre, and vitriol. Perhaps theſe different minerals are at the bottom only one principle, diſguiſed under theſe four ſalts, according as it has been mixed by nature with certain ſubſtances, or according as it has been managed by men. M. *Geoffroy* thinks, that the *Engliſh* and *Swediſh* allums may partake more of vitriol, and the *Italian* of ſea ſalt, which may cauſe a variation in ſome nice operations, or change the effect of ſome medicines, which require a great exactneſs.

IX. *Microſcopical obſervations on a cheeſe-mite.*

M. *Sauveur* imparted to the company a fact that M. *Forger* had writ to him from *Breſt*. M. *Mollard*, chief engineer, had ſhut up in a little common microſcope a cheeſe-mite to ſee what would become of it. This worm lived above ſeven months without taking any nouriſhment, unleſs the little air, which was in the microſcope, ſupplied it with any. It always moved ſenſibly, chiefly when it was expoſed to the ſun; then it turned and agitated itſelf a hundred different ways. At laſt it died, and from day to day, from white, it became red. This little carcaſs dried like a *chryſalis* of a ſilk worm, and at the end of 12 days, there came out of it a fly as big as the worm. It was not at all made like the common flies, but a little longer, and of the figure of thoſe which we ſometimes ſee about neceſſary houſes. It never took any nouriſhment, unleſs it was that of the *chryſalis* which it came out of. It died

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at the end of ten days, and afterward dried and waſted.

X. *Of two kittens joined together from the head to the navel.*

M. *Mery* ſhewed to the company two kittens, which were united in the belly of their mother They were joined from the head to the navel, and made in all this extent but one ſingle body; but in all the reſt, they were two very diſtinct, and well ſeparated. We ſhall not enter into a more particular detail of this monſter; it is eaſy to imagine in general, that two *ova*, or if they are not admitted to be *ova*, two little *fœtus's* in their firſt formation, finding themſelves of equal ſtrength, and alſo meeting too near in the *uterus*, may faſten and ſtick together, after which the liquors that ought to nouriſh and ſtrengthen them, being become common to them, they intirely quit, in one or other, certain courſes in which they would flow with difficulty, which abſolutely makes certain parts in one of the *fœtus's* periſh, and renders them ſingle for both, while theſe liquors flowing in the other parts of the two *fœtus's* with equal eaſe, keeps them always double. The meeting of the 2 *fœtus's* is only chance, and from certain directions of the veſſel more or leſs favourable to the courſe of the liquors, which makes them quit certain ways, and always follow others; and as this chance is ſuſceptible of an infinity of different combinations, the double monſters are infinite.

M. *Mery*'s two kittens are in another reſpect more worthy of the attention and ſurprize of philoſophers. They had but one *oeſophagus* and one *trachea*; but theſe two canals were joined in

ſuch a manner, that they made but one, and this ſingle canal had no communication but with the ſtomach, and no ways with the lungs, and conſequently was only one ſimple *oeſophagus*. The monſter could not therefore take in any air; and yet it lived about an hour after being out of the mother's belly.

XI. *On an ancient communication of the* Mediterranean *with the* Red-ſea.

M. *le Comte de Pontchartrain*, making uſe of his authority to aſſiſt the progreſs of the ſciences, had ſent into *Egypt* ſome memoirs made by M. *Deliſle*, which pointed out what was to be wiſhed, in order to rectify the map of that country. Theſe memoirs were accompanied with very ſtrong recommendations to the conſuls and vice-conſuls. It was in the execution of theſe orders of M. *de Pontchartrain*, that M. *Boutier* travelled over the whole *Delta*, and ſent to that miniſter a map of it, with a little account which explained it. M. *Deliſle*, to whom M. *le Comte de Pontchartrain* had done the honour to ſend the whole, ſpake of it to the academy.

The modern *Egypt* is but little known, altho' ſufficiently near and frequented; and we may reckon, that it is the ſame with all countries, where the inhabitants are in ignorance, and where learned ſtrangers ſeldom travel, at leaſt to make obſervations. Altho' M. *Boutier* has not travelled over the *Lower Egypt*, ſo much as it would be neceſſary, he has nevertheleſs, in M. *Deliſle*'s account, conſiderably reſtored the map, which was very much diſfigured. He begins with conſidering the *Delta* of the ancients, thoſe mouths which they have aſcribed to the *Nile*, the greateſt

part

part of which had been loſt by the ignorance of the modern geographers, a great number of towns, whoſe names are not yet much altered; for example, *Samanout*, or according to the *Coptes*, *Sebennetu*, which is the ancient *Sebennitus*, *Abboutſier*, or *Butſir*, which is *Buſiris*, &c. But what is moſt remarkable in this map, is an extremity of a canal, which goes out of the moſt eaſtern arm of the *Nile*, and which M. *Deliſle* judges to have been a part of that which formerly made the communication of the *Nile* and the *Mediterranean* with the *Red-ſea*.

As this ancient communication, which M. *Deliſle* eſtabliſhed for an indubitable fact, is unknown at this time, even by many of the learned, we were glad to ſee the proofs which he had of it; he gave them ſo plainly, and took the greateſt part of them in places ſo well known, that all the difficulty that remains is, to know how they have eſcaped the obſervation of every body.

Herodotus, in his ſecond book, ſays, there was in the plain of *Egypt*, a canal drawn from the *Nile*, a little above the town of *Bubaſte*, and below a mountain, which went on the ſide of *Memphis*, that this canal was extended very far from Weſt to Eaſt, that afterwards it turned to the South, and flowed into the *Red-ſea*; that *Nechus*, ſon of *Pſammiticus*, was the firſt that undertook this work, where 120,000 men had periſhed, that they had quitted it upon the anſwer of an oracle; but that *Darius*, ſon of *Hyſtaſpes*, had finiſhed it; that it was four days navigation; and that two gallies might paſs abreaſt in it.

Diodorus ſpeaks of it in the firſt book of his *Bibliotheca*, and agrees with *Herodotus*, except in this, that he makes the canal to be left unfi-

niſhed by *Darius*, to whom very bad engineers had repreſented, that the *Red-ſea*, being higher than *Egypt*, would overflow it; and in this, that he does not make the work to be finiſhed but by *Ptolomæus Philadelphus*. He adds, that this canal had been called the *river of Ptolomy*, that this prince had cauſed a town to be built at its outlet into the *Red-ſea*, which he had named *Arſinoe*, from one of his ſiſters that he loved, and that the canal might be opened or ſhut, according as it was neceſſary for navigation.

Strabo, lib. 17. of his *geography*, agrees with *Diodorus*, in every thing, it only remains to reconcile *Herodotus*, who makes the work to be finiſhed by *Darius*, with *Strabo* and *Diodorus*, who give that honour only to *Ptolomy*; but there might happen to a work of this nature, as ſoon as it was finiſhed, an infinite number of inconveniencies, which rendered it uſeleſs, till a new work was made.

At the point of the gulf, which they have called the *Red-ſea*, were two cities, *Heroopolis* and *Arſinoe*, which, according to *Strabo*, was alſo named by ſome *Cleopatris*. Beſides *Strabo*, ſpeaking of the expedition made into *Arabia* by *Ælius Gallus*, the firſt *Roman* governour of *Egypt*, ſays, that *Gallus* cauſed veſſels to be built at *Cleopatris*, near an ancient canal derived from the *Nile*. In another place, he ſays alſo, that *Heroopolis* was upon the *Nile*, and at the extremity of the *Red-ſea*.

After this, we may paſs over ſome other authorities, which were alſo related by M. *Deliſle*. Every body knows the deſign that ſome princes had of making this communication; every body knows, that it was croſſed by the chimerical fear of the inundation of the *Red-ſea*; and as if the greateſt

greateſt part of readers had been ſtruck with the ſame fear, they have not ſeen in authors the intire execution of the canal.

M. *Deliſle* has carried his inquiries even to the *Arabian* authors. *Elmacin*, lib. 1. cap. 3. ſays, that under the *Caliph Omar*, toward the year 635 of the chriſtian æra, *Amr* cauſed a canal to be made to tranſport the corn from *Egypt* into *Arabia*; probably he only renewed the old one, the navigation of which might eaſily have been neglected in the decline of the *Roman* empire. But in the year 150 of the *Hegira*, which agrees with the year 775 of *Chriſt*, *Abugiafar Almanzor*, 2d *caliph* of the *Abbaſſides*, cauſed this canal to be ſtopped on the ſide of the ſea. If ever this union ſhould be renewed, the world would change its face; *China* and *France*, for example, would become neighbours, and our poſterity would lament the fate of the barbarous ages, when the *Europeans* were obliged to go round *Africa* to get into *Aſia*.

XII. *Of the manner of cutting moulds for hyperbolical glaſſes, and for turning all conoids in general.*

The rays coming from a diſtant point, as the centre of the ſun, and for this reaſon thought to be parallel, having paſſed through a glaſs, which is a portion of a ſphere, do not reunite at all in a ſingle point. Their *foci* have ſo much more extent as the glaſſes are portions of greater ſpheres, and as they are greater portions of them.

It would not be the ſame with glaſſes, that ſhould be portions of ſolids, either conoid, elliptical or hyperbolical, provided nevertheleſs that the ellipſes or hyperbolas, of which theſe ſolids

ſhould

ſhould be formed, had a certain condition, that is, that the ratio of the great *axis* of the ellipſis to the diſtance of its *foci*, or the ratio of the diſtance of the two *foci* of the hyperbola to its determined diameter, was the ſame with the conſtant *ratio* of the ſine of incidence of a ray upon the ſurface of the glaſs, to its ſine of refraction in the glaſs. Then the rays of a diſtant point, which had paſſed through the elliptical or hyperbolical glaſs, would gather together in a ſingle point, which would be one of the *foci* of the ellipſis, or of the hyperbola.

This conſiderable advantage of reuniting into a ſingle point the rays from a ſingle point, has made M. *Deſcartes* prefer the ellipſes and hyperbolas to circles, and other particular reaſons have made him prefer the hyperbolas to the ellipſes. He had even given the deſign of a machine for ſhaping glaſſes into hyperbolas; but it did not appear convenient for practice, and we have contented ourſelves with ſpherical glaſſes, of which we only take ſuch a portion as reunites more rays in the ſame ſpace than any other portion, and reunites them in a ſpace ſo ſmall, as not to be ſenſibly more than one point. It is partly for this reaſon, that in the uſe of the great teleſcopes, we do not leave the ſurface of the object-glaſs entirely uncovered; we had rather receive fewer rays from the ſame point, and have them more exactly reunited. In the hyperbolical figure, a greater ſurface will not reunite the rays leſs exact into a ſingle point, than a leſs ſurface, and conſequently we ſhall have at the ſame time a perfect reunion, and as great a light as we pleaſe.

But it is well to obſerve, that the hyperbola would reunite in one point, only the rays from the ſingle point of the object, which would be in

its

its axis, and that all the rays from all the other points of the ſame object, would be ſo much leſs exactly reunited, as theſe points would be diſtant from the *axis*. On the contrary, circles which do not reunite exactly into a point the rays which come from any point of the object, reunite into an equal extent, and exactly in the ſame manner the rays which come from all the different points of the object, and conſequently the image of the object formed by the hyperbola, will be moſt lively and moſt perfect in the middle point; but in the other points it will be ſo confuſed, that it will not perhaps be any more an image, whereas thoſe which are formed by the circles, leſs lively and leſs perfect in the middle, are at leaſt equal in all their parts. Thus the ſpherical glaſſes are probably beſt for ſight, but the hyperbolical ones would have the advantage in burning; for one ſingle point is ſufficient for this effect.

However, as M. *Deſcartes* was deſirous to apply hyperbolical glaſſes to teleſcopes, and as the hyperbola has at leaſt the advantage in burning, M. *Parent* was not willing to leave the properties of this figure uſeleſs, and thought of a way of bringing them into practice. In the firſt place, there muſt be hyperbolical moulds, ſome convex and other concave, againſt which the pieces of glaſs muſt be ground, which will take one of theſe figures.

But firſt, it is not eaſy to ſhape theſe moulds into hyperbolas at one ſingle ſtroke; for if this figure is given them only by trying, and placing one point after another, we ſhould make a work not very exact, but uneven and irregularly curved. 2. As we are accuſtomed to hyperbolas, of which the determined diameter has to the diſtance of the *foci*, the *ratio* of the ſines of refraction to thoſe of incidence,

cidence, the moulds muſt be made ſo, that the conſtruction may preſerve this proportion with extreme exactneſs. 3. We muſt take care, that the figure of the moulds do not alter by the ſame motion, and by the friction which we uſe to grind the glaſſes, which ſhall be applied to them.

M. *Parent* has ſhewn what method he took to overcome theſe difficulties, he has alſo done more than his deſign obliged him to do, he has found a practice for turning upon the common turn, and without a model, all ſorts of conoids, that is of ſolids, formed by the revolution of ſome conic ſection about an axis, which embraces the hyperbolical mould as a ſort of ſpecies. But without taking the round of the general method, that which is for the hyperbolical moulds in particular is reduced to holding againſt the ſurface of the mould a ſolid rule, which makes with the *axis* an angle of which the tangent is to the whole ſine, as the great *axis* of the hyperbola ſought is to the ſmall ones.

XIII. *Of the reduction of the motions of animals, to the laws of mechanicks, tranſlated by Mr.* Chambers.

The ſame laws reign every where. The works of nature turn on the ſame principles as thoſe of art; and when I barely lift up my arm, there is a power raiſing a weight by means of a lever.

This mechaniſm, tho' concealed from the eyes, and rendered ſtill more inſenſible by the facility of natural motions, is not the leſs real. When my arm, or rather that part thereof between the elbow and the wriſt, having before been pendant, comes to be raiſed on high, it moves circularly about the elbow, or rather about a point to be

conceived in the centre of the articulation of the upper joint of the arm with the lower. This motion is effected by means of a muſcle ariſing out of the former, inſerted into the latter; which ſwelling, and conſequently ſhortning, raiſes up the arm, and thus overcomes not only the weight of the arm, but that of the hand likewiſe, which we compute at 6 *lb.*

If the arm and hand were a mere right line, they would only make a lever, whoſe fixed point might be ſuppoſed in the centre of the elbow: but as they have a ſenſible bulk or thickneſs, they are at the ſame time a weight to be ſuſtained.

On this ſuppoſition, if their weight or thickneſs be every where equal, their centre of gravity will be in the middle of their bulk, that is about 8 inches diſtance from the elbow: ſo that here we are to conſider a weight of 6 pounds, ſuſpended at 8 inches diſtance from the fixed point.

If the muſcle that acts be fixed to the arm an inch below the elbow, and pull perpendicularly upwards, while the weight draws perpendicularly downwards, the power is only one inch diſtant from the fixed point, while the weight is diſtant 8 inches. And as this weight is 6 *lb.* the power muſt be 48 *lb.* to ſuſtain it, and more to raiſe it.

But the power does not pull perpendicularly. The muſcle is fixed very obliquely to the arm, and conſequently retains the ſame obliquity of direction when it acts; and as the diſtance of a power from the fixed point is meaſured by a perpendicular, drawn from the fixed point to the direction of the power; and this perpendicular is ſo much ſhorter, as the direction is more oblique; the ſtrength of the muſcle will be required conſiderably to exceed 48 *lb.* If the perpendicu-

lar, drawn from the fixed point to the direction, be only equal to $\frac{1}{2}$ the distance from the insertion, that is, be only $\frac{1}{2}$ an inch, the force must be 96 *lb*. Who would imagine, that in only lifting up the arm, we employ a force of 96 *lb*.

What surprizes most is, that nature, notwithstanding her usual oeconomy, which never fails to take all possible advantages, should here, contrary to the rules of mechanicks, place the power between the fixed point and the weight; and thus make it draw obliquely: which gives it the utmost disadvantages, and obliges it to be incomparably greater than the weight. — Which same supposition being that most usually in muscles would almost seem at first sight, that the sovereign wisdom had here forgot itself.

But let it be considered, that a small power applied advantageously to a lever to raise a great weight, must have a great velocity, and pass over a deal of space, while the weight only raised a little and slowly. On the contrary, a great power applied to its lever disadvantageously, and having only a small weight to sustain, travels over a small space, while the weight travels a deal.

If now a large bell, for instance, be to be raised to the top of a steeple, 'tis enough generally it be raised thither, nor matters it much in what time. And as any powers we have to use are small and inconsiderable, compared to the largeness of the weight, 'tis a fine advantage to have the bell rise some 20 or 30 fathom in a day, while *e. g.* the men who raise it travel perhaps some leagues. But if my hand, considerered as a weight, be to move for some office useful or necessary for the preservation of life; the great business usually is to have it move quickly not to spare strength; which consisting in the contraction of a muscle,

and

and the quantity of ſpirits that ſwell it, is always greater than needs for the ordinary motions.

'Tis true, nature might have given the ſame velocity to the weight, by having the muſcle faſtened at the ſame diſtance from the fixed point; and yet have ſaved ſomething in the ſtrength of the muſcle, by giving it a perpendicular direction. But 'tis clear, that in this direction, it would have required a deal more room, than when couched or laid, as it now is, along the bone it is to pull. Now an animal being an aſſemblage of an infinite number of different machines, the ſaving of room is a very material conſideration.

Beſides, the ſtrength of a muſcle depends on the quantity of ſpirits, whereby it is put in contraction, ſo that the advantageous direction of a muſcle, could in reality have only ſerved to ſave ſomething in the quantity of ſpirits. Now a great quantity of this juice was indiſpenſibly neceſſary on other accounts, particularly for the offices of ſenſation; ſo that the exceſs of ſtrength is provided for without any particular expence.

From the whole, it appears, that tho' the mechaniſm of animal motion departs ſomewhat from the common mechanicks, in reſpect of the poſition of the moving power, yet it is finally reducible to the ſame laws: and the ſtrength of a muſcle may be exactly computed, provided we know the weight it is able to ſuſtain, the fixed point on which it moves, and the diviſion wherein it acts.

Now the weight a muſcle ſuſtains, is either the mere weight of the part it draws, or that weight with ſome foreign load ſuperadded thereto. Thus by the weight ſuſtained by the muſcle, which bends the arm, we either mean the natural weight of the hand, and the firſt joint of the

arm; or beſide this weight, the greateſt foreign weight which can be bore in that action. And take which way you will, 'tis experiment only, tho' an experiment eaſy made, that muſt determine what this weight is.

But there is ſome difficulty in finding the fixed points, and directions. The mechaniſm of theſe motions is ſo complicated, that the application of rules thereto become very precarious, or at leaſt difficult; the intelligence which directed theſe works, not having confined itſelf to thoſe ſimple caſes, which we are accuſtomed to.

The celebrated *Borelli* was the firſt that engaged in enquiries of this kind, in his treatiſe *De Motibus Animalium*; a work truly laudible, both for the merit of being the firſt, and for the many curious truths diſcovered therein. Yet M. *Parent* hath ventured to charge it with want of exactneſs in certain points, and even gives us a new general theory, whereby a further degree of light is brought into the whole matter; a little ſketch whereof we here ſubjoin.

When the concave end of one bone, receives the convex end of another, and this ſecond being pulled by a muſcle, moves upon the firſt, if their figures be ſuch, that during the whole motion the ſecond only bears upon the firſt by a ſingle point: it is evident, that in caſe of an equilibrium produced by an equality of two oppoſite powers, their common direction would paſs through this point, which therefore is the fixed point. And it is evident withal, that in the motion of the bone, this point is continually changing place, which is one of the peculiarities of theſe mechanicks.

If the concavity and convexity of the two bones were perfectly ſpherical and concentrical, they would touch in all their points; and yet ſtrictly

ſtrictly ſpeaking, the two ſurfaces would reſt or bear on each other only in one point of their circumferences, which point would be the ſame, as if, having been neither ſpherical nor concentrical, they were rendered ſo in ſome inſtant of their motion, it being viſible, that this would make no alteration in the point of ſupport. Accordingly, tho' the common centre of two concentric ſpheres, be really immovable, during their motion, it is not properly and abſolutely the point of ſupport, but that which is a point of the circumference, which is continually ſhifting every moment of motion. 'Tis therefore from this point that we are to meaſure the diſtances of the muſcle, and of the weight which pull againſt each other. 'Tis true, in meaſuring them from the common centre of the 2 ſpheres, as *Borelli* has done, we come at the ſame concluſion, by reaſon this common or compound direction does alſo paſs thro' the centre: but this is only by accident, which would not obtain in other figures: and it is neceſſary to know preciſely and univerſally where the real point of ſupport is found.

This being had, the directions of the weight and the muſcle muſt likewiſe be found, in order to draw perpendiculars to the ſame from the point of ſupport.——— As to the weight, its direction of courſe is the vertical line, whereby it tends downwards. But the muſcle being frequently traverſed by other muſcles, its direction in ſuch caſes is compounded of its own, and that of the others, ſuppoſing them to act at the ſame time. However, as they terminate in the ſame tendon, which is their common cord whereby they pull; this tendon naturally repreſents their compound direction, which therefore is eaſily found.

Thus

Thus much only regards the ſimple articulations, that is where only two bones are joined together to perform ſome motion. When there are ſeveral bones, as for inſtance, the vertebræ of the ſpina dorſi, which conſpire to bend the back inwards, the articulation is compounded: and the application of the rules may be ſaid to be ſo likewiſe, tho' at bottom they amount to the ſame.——When the back is bent, we may ſuppoſe that too contiguous vertebræ, which before touched in the whole ſurface of one of their ſides, begin to ſeparate or open outwards, and continue to widen further and further, as their diſpoſition will admit; and during ſuch motion, only touch by a ſingle line which remains common to them in their baſe, in the middle of which is the fixed point.

But in fact, the caſe is not ſo ſimple. The bones which move by withdrawing them before contiguous ſurfaces, leave no vacancy between them. They are bound together by cartilages, which may be conceived as adhering, on both ſides, to their whole ſurface. Theſe cartilages, being dilatable and compreſſible, neceſſarily dilate as the bones tend to divaricate. And for as much as this dilatation requires a certain force, the muſcle which produces the whole motion, muſt have this over and above what is required merely to raiſe the weight.

To eſtimate the power neceſſary to dilate the cartilage, M. *Parent* is obliged to conſider the reſiſtance which the cartilage makes to a dilatation, as a power applied at a certain diſtance from the fixed point, and pulling againſt the muſcle. And by reaſon the dilatation is greater in different parts of the ſame cartilage, in proportion as they are farther diſtant from the fixed point; the dilatation may be conſidered as velocities, and be

taken for the point where the whole force is united; which would be the centre of agitation of the plane of the cartilage.

If two or more bones be ſo diſpoſed, that upon opening them, or their upper ſide, they approach on the lower; and of conſequence dilate one half the cartilage, which binds them, and compreſs the other; the fixed point will be in the middle of the cartilage: and the points whereon the reſiſtances of the cartilages both to their being compreſſed and dilated unite, would be found in each of their halves, after the ſame manner as the point of union of a ſingle reſiſtance would have been in the whole.

A compound articulation having ſeveral fixed points, there muſt be the ſame number of perpendiculars drawn from each, both to the direction of the weight, the direction of the muſcles whether ſimple or compound, and thoſe of the cartilages.

M. *Parent* divides the compound articulations into conſecrative, where all the motions tend the ſame way, alternate, where they are by turns different ways: Theſe laſt motions are eaſily reduced to the laws of the firſt.

From this theory may the ſtrength of that prodigious number of machines, which are played either ſeparately or together in the body of an animal, be computed, and the relation of one to another, either preciſely or pretty nearly, be found. If we could but likewiſe enter into all the views which required ſuch different relations of powers, and the advantages ariſing from them, what intelligence would not be confounded therewith?

XIV.

XIV. *Of the reſiſtance of hollow and ſolid cylinders.*

If this queſtion was propoſed, which of two ſticks equal in length, and cylindrical, is the eaſieſt to break upon the knee, that which is entirely ſolid, or that which is hollow, having the ſame quantity of matter with the other, moſt people would not heſitate at all to decide that the hollow ſtick would be the eaſieſt to break.

Nevertheleſs it is quite the contrary, as ſoon as we conſult the principles of mechanicks. When we reſt a ſtick upon the knee to break it, we reſt it by ſome one of its points, and it is the point diametrically oppoſite, which will take a circular motion about the point of ſupport, while the fracture is made. Here then is a lever, and this point which moves circularly, deſcribes an arch ſo much the greater, as it is farther diſtant from the point of ſupport, or from the fixed point, and conſequently it has ſo much more force to reſiſt the power, which tends to make the fracture. A thicker cylinder full is then more difficult to break, not only becauſe it contains more matter, upon which we muſt act, but alſo becauſe the diameter of its baſe is greater, and the extremity of its diameter, which moves in the fracture, is farther diſtant from the fixed point. If this cylinder preſerving the ſame quantity of matter, became hollow, it is viſible, that its total diameter, that is, the diameter, as well of the hollow part, as of the ſolid, would neceſſarily increaſe, and conſequently alſo one of the cauſes, which made its force, and its reſiſtance againſt being broken.

Every

Every hollow cylinder is then ſtronger than a full cylinder, which has only the ſame quantity of matter; and this, according to all appearance, is one of the reaſons why the bones of animals and the culms of corn and graſs are hollow.

Galileo, the firſt author of theſe ſorts of inquiries, has conſidered in the full and hollow cylinders, having their baſes formed of the ſame quantity of matter, only the inequality of their diameters, and conſequently he has eſtabliſhed, that the reſiſtance of a hollow cylinder, is to that of a full cylinder, as the total diameter of the hollow one is to the diameter of the full one.

But this conſideration is imperfect in this, that the extenſions of the fibres of which the cylinders are compoſed, are not taken in. Theſe extenſions, and conſequently the reſiſtances of all the particular fibres, continually increaſe from the fixed point to the moſt diſtant fibre, which muſt break the firſt, and which we may ſuppoſe in the greateſt extenſion that it can ſuffer. It is the ſum of all theſe unequal reſiſtances that makes the reſiſtance, which all the fibres together oppoſe to the power, which tends to break them.

Thus the total reſiſtance of the cylinder depends upon three things; on the quantity of matter that compoſes the baſe; on the reſiſtance that all theſe fibres together bring to their extenſion, and on the magnitude of the diameter of the cylinder.

There remains to determine and expreſs geometrically theſe magnitudes; and it is this that M. *Parent* has done. The circles of the baſe of the full cylinders muſt be made equal, to the full bands or zones of the hollow cylinders, and we muſt find the infinite ſum of the unequal re-

ſiſtances of all the fibres, which is a particular caſe of the general method of M. *Varignon*.

M. *Parent* being arrived to a general *formula*, which contains all the poſſible reſiſtances of hollow cylinders, compared to the full, has calculated upon this *formula*, a table, where he ſuppoſes that the total ſemi-diameter of a hollow cylinder is always 100 parts; and that the reſiſtance of the full cylinder, which contains in its baſe as much matter as the other, is alſo divided into 100 parts. We ſee by the table,

1. That in proportion as the hollow cylinder, of which the *radius* can have but 100 parts of a certain determinate magnitude, has more void, and conſequently leſs matter, it makes a greater reſiſtance than the correſpondent full cylinder. 2. That this inequality of reſiſtance always diminiſhes in proportion as the hollow cylinder is leſs hollow, and contains more matter; that, for example, a cylinder, of which the void is 99 *radius*, and 1 in thickneſs, and to which conſequently a full cylinder anſwers, which is only 14 *radius*, has a reſiſtance, which is to that of the full cylinder, as 848 to 100, that is, as $8\frac{1}{2}\frac{2}{5}$ to 1; and that the cylinder, which has 50 in void and 50 in thickneſs, and to which a full cylinder of 87 *radius* anſwers, has a reſiſtance, which is to that of the full one, only as 121 to 100. 3. That the hollow cylinder of 99 void, the reſiſtance of which, compared to that of the full one, which is 14 *radius*, would be, according to *Galileo*, $7\frac{1}{7}$ times greater, has one $8\frac{1}{2}\frac{2}{5}$ times greater according to the hypotheſis of M. *Parent*, which is alſo that of M. *Mariotte*.

XV. *Of a machine invented by F.* Sebaſtien*.

This machine, invented by F. *Sebaſtien*, was only made to try if the fall of bodies followed the proportion of *Galileo*; or rather to ſhew by experiment, that it did follow it; for the machine was ſolely conſtructed upon this hypotheſis: it was formed by the revolution of a parabola about its axis, and the circumferences of circles of ſmall ſpiral planes, which were the different ſpaces run over by the falling bodies, repreſented the ſeries of uneven numbers.

But as it is not abſolutely impoſſible to eſtabliſh, or at leaſt to be willing to try ſome other hypotheſis than that of *Galileo*, upon the fall of bodies, M. *Varignon* found the thought of F. *Sebaſtien* too ingenious not to extend it to all the hypotheſes imaginable.

Whatſoever hypotheſis therefore we take upon the fall of bodies, M. *Varignon* requires us to expreſs by the ordinates of a curve, the different velocities acquired at each inſtant, that afterward we cauſe this curve to make a revolution about its axis perpendicular to the horizon, like that which the parabola makes in the hypotheſis of *Galileo*; and laſtly, that round the ſolid, formed by this revolution, we direct from the top to the bottom an inclined plane, which makes always the ſame angle with the curve, that it always meets after it has formed the ſolid; after that, he demonſtrates that if the hypotheſis that we have taken is the true one, a body that ſhall fall from the top of this machine, by the inclined plane, will make all its revolutions round the machine, altho' unequal, in equal times, which happened at leaſt ſenſibly, in that of F. *Sebaſtien*.

* See *pag.* 30 of this abridgment.

The eſſential principle of this property of the machine is the perpetual equality of the angles of the inclined plane with the generating curve. From this equality all geometricians will very eaſily conclude, that all the different portions of the inclined plane, taken between the ſame arches of the ſolid, and if we may uſe the expreſſion, between the ſame meridians, are always between themſelves as the ordinates of the curve which anſwers to them. Now theſe ordinates expreſs the acquired velocities, and the portions of the inclined plane are the ſpaces run over by the means of theſe velocities; therefore the ſpaces are always as the velocities, therefore different ſpaces are run over in the ſame time.

In F. *Sebaſtien*'s machine, all the angles of the inclined plane, and of the arches of the curve, were very near right ones, which was ſufficient for the ſenſible equality of the time of the fall.

M. *Varignon* eaſily found, that in his general machine, the length of the inclined plane will always be to that of the general curve, as the total ſine to the ſine of the complement of the angle of the inclined plane.

AN ABRIDGMENT OF THE PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris* for the year 1702.

I. *Observations on the quantity of rain which fell at the royal observatory in* 1701, *with some remarks on the thermometer and barometer. By M.* de la Hire *.

THE year 1701 seemed extraordinary for the dryness of the spring; it was however in general one of the most rainy years that we have had a long time. For there fell in the months of

	Lines.		*Lines.*
Jan.	17 ½	July	27 ½
Feb.	19 ¼	Aug.	45
Mar.	22	Sept.	10
Apr.	1	Octob.	24 ¾
May	20 ½	Nov.	19 ¼
June	38 ½	Dec.	10 ¾

The sum total is 256 lines ¼, or 21 inches 4 lines ¼.

We find hereby that there was no rain in *April*, and that this might have rendered the year very unfruitful, if the earth had not been moistened by the rains of the three preceding months. For the snows which usually fall in winter, and remain upon the ground in this season, hardly penetrate

* Jan. 7, 1702.

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it at all, and there would be need of a very great quantity to ſupply as much water as fell in the three firſt months of this year; for the 5 inches of water of theſe months muſt have been ſupplied by 2 feet ½ of ſnow, which would have been very extraordinary, without reckoning, that the greateſt part of the ſnow dries before it is melted, eſpecially in winter when the air is very dry; and this is what cannot happen to the water, which has entered into the earth, and has penetrated it long before.

The three months of *June*, *July*, and *Auguſt*, have commonly ſupplied almoſt as much water as all the reſt of the year; but theſe great ſummer rains are quickly diſſipated by the great heat of the air, and by the dryneſs of the ground.

The cold was not conſiderable; for it hardly froze. My thermometer marks the beginning of the froſt, when it is at 30 degrees, and it fell at the loweſt but to 28 ½, whereas, in a great cold, ſuch as we have ſometimes in this country, it falls to 7 degrees, as it was in *Feb.* 1695.

Towards the end of *Jan.* and beginning of *Feb.* 1701, which was the coldeſt ſeaſon, my thermometer was often at 40 degrees, which is not very diſtant from the mean ſtate of the air, as I have found upon leaving the ſame thermometer at the bottom of the cave of the obſervatory for ſome days, where the liquor always kept at 48 degrees. We may alſo obſerve, that on the laſt day of *Nov.* the heat was as great as on the 12th of *June*.

The heats of the months of *July* and *Aug.* were extraordinary; for the ſame thermometer roſe pretty often to 65 degrees, and *Sept.* 1. it was at 65 degrees ½ at the higheſt. This thermometer is always expoſed to the air, but in a place much

ſheltered, and not expoſed either to the wind or ſun; and all the obſervations that I have made on it, are always about the riſing of the ſun, which is the time of the day when the air is moſt cool: for the hotteſt time is generally at 3 in the afternoon. Wherefore to know the greateſt heat of the air, in a place not affected by the ſun, I have obſerved, that my thermometer roſe to 77 degrees $\frac{1}{2}$ with a ſtrong S. E. wind, *Aug.* 17. at 3 $\frac{1}{2}$ in the afternoon, which is a mark of extreme heat. This thermometer is very long, and may be expoſed to the hot ſummer ſun, without the liquor's riſing to the top of the tube, that I may mark the more eaſily upon it the degrees of heat and cold, even when it is expoſed to the ſun.

We may hence conclude, that the cold of the air in this country is in general greater than the heat in the abſence of the ſun. For the mean ſtate of the air being 48 degrees on my thermometer, and the greateſt heat 77, there are but 29 degrees of difference, which being taken from 48, there would remain 19 degrees for the mark of a cold at the ſame degree below the mean that the heat is above it; and yet it ſometimes happens here, that the ſame thermometer falls to 7 degrees.

We may obſerve, that the greateſt heat of the day does not always follow the heat of the morning, as may be ſeen in theſe obſervations: for the greateſt heat of the morning was *Sept.* 1. the thermometer marking 65 degrees $\frac{1}{2}$, and that of the afternoon *Aug.* 17, and on that very day it was in the morning a little leſs than on *Sept.* 1. for the thermometer marked but 63 degrees, which may happen from ſeveral particular cauſes.

The barometer which I uſe is ſimple, as uſual, having a tube of moderate thickneſs, that the quickſilver may have the more liberty to move

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in it; the bottle at the bottom is proportioned to the bigneſs of the tube, ſo that the falling or riſing of the quickſilver of 1 inch ½ in the tube, is not ſenſible in the bottle. This barometer is always placed at the height of the great hall of the obſervatory, which is near 22 toiſes above the level of the ſurface of the river in a mean ſtate. I obſerved it was at the loweſt this year at 26 inches, 10 lines *March* 6. and the higheſt at 28 inches, 2 lines ½ *Feb.* 9. The difference therefore between the higheſt and the loweſt was but 1 inch, 4 lines ½, which is ſomething leſs than the common, which is 1 inch, 6 lines.

But the moſt conſiderable thing that has happened this year is the hurricane of *Feb.* 2. the wind was very violent, and the barometer was in an almoſt mean ſtate at 27 inches, 4 lines ½, and there was but 1 line ½ of rain, which may be obſerved as an extraordinary thing; for in great motions of the air, the barometer falls very low.

I found the declination of the needle to be 8° 48′ *Sept.* 22. W. variation. I made uſe alſo of the ſame compaſs, of which the needle is 8 inches long, and very well ſupported on a very ſlender pivot. I always make the obſervations againſt one of the pillars of the lower terraſs of the obſervatory, by applying the ſide of the box to it, wherein the needle is incloſed, and by this means I avoid all the errors that could happen from the poſition of the compaſs on the meridian. I have formerly proved the poſition of the ſide of this pillar, by the ſun's paſſing the meridian, applying a great rule to it, which had at its extremities two ſights for the rays of the ſun to paſs thro'; the aperture of the objective ſight, and the tract marked on the other, were in a line exactly parallel

rallel to the ſide of this rule, which was applied againſt the face of the pillar.

II. *A diſcourſe on ſome properties of the air, and the means of knowing the temperature of it in all the climates of the earth. By M.* Amontons *.

The experiments, which I have made three years ago on the dilatation of the air by the heat of boiling water, ſhewed me, that unequal maſſes of air loaded with the ſame or equal weights, augmented equally the force of their ſpring by equal degrees of heat; and as my principal end in theſe experiments was to know, how much the heat of the boiling water increaſed the ſpring of the air above what it has in the water, which we call cold, theſe experiments led me at that time to think, that it was but in a quantity capable of ſuſtaining 10 inches of quickſilver in height beyond the weight of the atmoſphere: but having ſince carried my experiments farther, I have found, that the ſpring of the air augmented by the heat of boiling water, was not fixed to ſuſtain no more than 10 inches of quickſilver, beſides the weight of the atmoſphere; but that it ſuſtained more or leſs in proportion to the weights with which it was loaded, and that this augmentation was always about the third part of theſe weights, when the air is at firſt in the ſtate which we here call temperate, and leſs than a third part when the air is hotter; and on the contrary, more than a third part when it is colder. For inſtance, if, when it is temperate, a maſs of air loaded with 30 inches of quickſilver, comprehending the load of the atmoſphere, has augmented its ſpring by

* June 28, 1702.

the heat of boiling water, ſo as to ſuſtain 10 inches of quickſilver, beſides the load equal to 30 inches of quickſilver: when this maſs ſhall be loaded with 60 inches, it will augment its ſpring 20 inches, and 30 inches when it ſhall be loaded with 90, and ſo of the reſt. Whence in ſeems, that we may draw this conſequence, that *the ſame degree of heat, let it be ever ſo little, may continually increaſe the force of the ſpring of the air, if this air is continually loaded with a greater and greater weight.* And as we have already obſerved, that unequal maſſes of air equally augment the force of their ſpring by equal degrees of heat, we may draw this other conſequence, that *a very ſmall parcel of air, let it be ever ſo little, may acquire a greater elaſtic force, and greater and greater continually by a very ſmall degree of heat, if this little parcel is continually loaded more and more.* Theſe properties of the air may perhaps hereafter ſerve us, to explain ſeveral phyſical effects, of which we do not at preſent know the cauſes.

I have juſt now ſaid that experience had taught me, that unequal maſſes of air, loaded with equal weights, augmented the force of their ſpring equally by equal degrees of heat, and that the elaſtic forces which they acquired, were ſo much more conſiderable, as the weights with which they were preſſed were great; the reaſon of which is, that the maſſes of air, either being in the ſame *medium*, or conſidered as ſuch, and loaded with equal weights, there is no reaſon why one ſhould acquire a more conſiderable elaſtic force than the other. For tho' it is true, that if theſe maſſes of air had the liberty of extending themſelves, the greater would increaſe their bulk more than the ſmaller; this ought not however to take place in the augmentation of their ſpring, ſince, accord-

ing to M. *Mariotte*'s rule, unequal maſſes of air equally loaded muſt reduce themſelves to bulks proportioned to their firſt maſſes, to acquire new equal degrees of elaſtic force; and that, by the reverſe of this very rule, if equal maſſes of air unequally loaded have a liberty of extending themſelves, they will really poſſeſs ſpaces proportioned to the weights with which they are loaded; but not being able to extend themſelves, they muſt neceſſarily acquire elaſtic forces proportioned to the ſame weights.

When I had diſcovered theſe truths, I endeavoured to apply them; and thought I could make an advantageous uſe of them in bringing thermometers to perfection.

Few perſons are ignorant, that the firſt thermometers made with air acted not only by the heat and cold of the external air, but ſtill more by its greater or leſs weight, and that the motion of theſe thermometers, cauſed by the weight of the air, was at leaſt as ſenſible as that which was cauſed by the heat, which rendered the obſervations on them uncertain, and conſequently of no uſe. It is true, that there have ſince been invented thermometers of ſpirit of wine, ſealed hermetically, which ſeem to act only by the alterations of the air, as to cold or heat: but beſide the ſpirit of wine's not receiving the impreſſion ſo quickly as the air, and the great maſſes receiving it more ſlowly than the leſſer ones, it is almoſt impoſſible for their tubes to be equal from one end to the other; whereby the ſame quantity of liquor, which towards the bottom might perhaps poſſeſs no more than 40 parts of their graduation, when driven up towards the top may ſometimes poſſeſs 45 or 50 more or leſs. Whence it comes to paſs, that if theſe thermometers were regulated only

according to the greateſt heat and greateſt cold of a climate, the temperate degrees of the thermometers would be all different from each other, tho' they ought to be exactly the ſame. But farther, let us ſuppoſe, what is not true, that theſe thermometers have none of the above-mentioned defects; what is a degree of heat of theſe thermometers? what knowledge do theſe degrees give us of the temperature of our climate? It is certain, that they give us none; the firſt of theſe thermometers were graduated, juſt as it happened, on the greateſt heat and cold of ſome years, and can only ſerve at moſt to ſhew us, that ſome are hotter or colder than others: which is of no great uſe, as we cannot certainly know the difference, and theſe inſtruments are not at all proper to tranſmit to poſterity the obſervations, that may be made on the different temperature of the climates: for to ſay, for example, that laſt year the thermometer roſe 7 or 8 parts more than the preceding, is giving no better intelligence how much this year was hotter than the other; than if one ſhould tell a perſon, who is ſolicitous about knowing the length of a pendulum that ſwings ſeconds, that it is as long as ſuch a ſtick, of which he does not know the length: but if one ſhould tell him, that the length of this pendulum is 3 feet, 8 lines $\frac{1}{2}$; then as theſe meaſures are known and fixt by uſe, and by the compariſon that may be made of them to all ſorts of dimenſions; he has no longer any reaſonable doubt, that may require to be cleared up. It is not ſo with a degree of the thermometers that have hitherto appeared; we cannot ſay that there is, for example, the 100th part of the difference between the greateſt heat and the greateſt cold of one year, ſeeing theſe differences are hardly ever equal; and if they were ſo, it would be

be at moſt but for a certain climate; thus a degree of the thermometer cannot be compared to any degree of heat, and conſequently cannot be the meaſure of it. On the contrary, if I ſay the greateſt heat of laſt ſummer was, for example, $\frac{6}{7}$ of that of boiling water, this degree of heat being known by thouſands of daily effects, that which I would have known becomes ſo alſo, and I can draw all the conſequences from it that I want. We muſt then agree upon a certain degree of heat, conſtant and invariable, known by every body, to which we may compare, and which comprehends all the other degrees of heat that can be in the air which we breathe. This is probably what the late M. *Colbert* intended, when he had a project of having a conſiderable number of thermometers conſtructed, and to ſend them into different parts of the world for obſervations to be made upon them: but probably that great miniſter gave over the deſign, only becauſe he juſtly thought, that the ſpirit of wine thermometers, ſuch as they had then, were improper for the purpoſe, and that it would have been almoſt impoſſible to eſtabliſh a ſufficient uniformity in theſe thermometers.

The degree of heat, neceſſary to eſtabliſh an uniformity in the conſtruction of thermometers, might be that of common boiling water, experience having ſhewn me, that it cannot acquire a greater degree of heat, let it be ever ſo long on the fire, and let the fire be ever ſo ſtrong.

* ABCD is one of the glaſs tubes, which I made uſe of for ſome former experiments, to know the increaſe of the ſpring of the air by the heat of boiling water, open at A, bent at C, and ending in a ball D. The bigneſs of this tube is

* Plate XVI. Fig. 1.

about

about $\frac{1}{2}$ line within, that of the ball 3 inches $\frac{1}{4}$, a little more or leſs without conſequence; and herein theſe thermometers have a great advantage above others, by the equality of their motion, ſo eaſy to find in theſe new thermometers, ſo difficult to find in the old ones; the length of this tube from A to B ſhall be 46 inches, that the total AC may be about 48. Let there be quickſilver from the entrance E of the ball, and in all the reſt of the tube quite to the aperture A; ſo that the ball D being in the boiling water, the air which it incloſes may ſuſtain by its elaſticity 73 inches of quickſilver, comprehending the weight of the atmoſphere, which is always ſuppoſed equal to 28 inches, and only 45 inches without comprehending it, beginning at the level of the quickſilver, which ſhall be at E; then the ſurface of the quickſilver in the tube AB, near the aperture A, ſhall be the term from whence we may begin to reckon all the other degrees of heat, which ſhall be leſs than that of boiling water: for as there is no climate upon the earth, that can be ſuppoſed to be as hot as boiling water, we ſhall conſequently have as high a degree of heat, as is known in any country, which will comprehend all below it, and from which we may begin to reckon. So that to expreſs the greater or leſs degree of heat of any climate, we need only count the number of inches and lines, by which the ſurface of the quickſilver towards A, ſhall be lower than the place to which the heat of the boiling water had made it riſe, having a regard alſo to the weight of the atmoſphere at the time of the obſervation, whether it is more or leſs heavy than 28 inches of quickſilver; becauſe the ſurface of the quickſilver towards A, will be too low by the quantity by which the weight of the atmo-

ſphere

ſphere ſhall exceed that of 28 inches of quickſilver, or too high by the quantity which ſhall fall ſhort of thoſe 28 inches. Wherefore, in the firſt caſe, we muſt ſubſtract this exceſs of inches and lines, contained from the degree of the heat of the boiling water; and, in the ſecond caſe, we muſt add it. It will be eaſy therefore, by the aſſiſtance of theſe thermometers, to know the temperature of all the climates of the earth, and to conſtruct other thermometers with ſpirit of wine for each climate, which may be compared with theſe new air thermometers. The degrees marked on them will be no longer unknown, and we may tranſmit our knowledge to poſterity, to obtain the advantageous uſes from them, which there is room to promiſe, not only for all philoſophical uſes, but alſo for our own preſervation. The thermometer muſt be prepared after the following manner.

* ABCD is a little bit of aſh, walnut, or any other wood of like nature, about an inch ſquare, and $\frac{1}{2}$ inch in thickneſs; in the thickneſs of which holes muſt be bored from ſide to ſide, as EF about 3 or 3 lines $\frac{1}{2}$ in bigneſs, and another of like bigneſs as GH, to communicate only with the firſt, and no farther. Then apply with ſome maſtick at G a glaſs tube † ILG, about 4 feet long, open at both ends IG, and bent at L, about an inch from the extremity G; apply afterwards at F another tube as FMNO, open alſo at the two extremities F and O, ſwoln toward F into a ball M, of about two inches in diameter, at $\frac{1}{2}$ inch from the extremity F, bent at N as near the ball M as poſſible, and deſcending again towards O with 6 or 7 inches; apply at E another end of a tube only 2 or 3 inches long:

* Plate XVI. Fig. 2. † Fig. 3.

all

all theſe tubes muſt be but about a line or two on the inſide, except the extremity O, which muſt be a little widened to receive the other tubes more eaſily that are to be applied to it. Theſe 3 tubes muſt be ſo applied to the little piece of wood with maſtick, that the quickſilver let in with a funnel at I may flow freely towards F and E, and may paſs, according as it ſhall be neceſſary, either thro' the tube FMNO, or the tube EP. Obſerve to cover this whole piece of wood well with maſtick, for fear the quickſilver ſhould get thro' its pores.

This little machine being thus prepared, apply it againſt a wall, making the ball M reſt upon two nails, and tying the tube * IL looſely, a little below its extremity I, with a piece of packthread to another nail faſtened alſo in the wall. Apply alſo with ſome maſtick at O the extremity A of the thermometer, into which you would introduce the quickſilver, making the bottom of it lean upon ſomething ſolid, as at C, after which cloſe the extremity P with maſtick, and then with a funnel pour the quickſilver in at the extremity I, which will gradually fill the ball M, and proportionably condenſe the air of the ball D. When the ball M is quite full, and the quickſilver begins to paſs by the bending N, and go down to C, leave off pouring in the quickſilver, and open the extremity P by heating it with the flame of a candle, driven thro' a little pipe, as when we ſeal hermetically, then withdraw the quickſilver from the ball M by the extremity P; and if the quickſilver in the tube AC is about 27 inches above E †, when the heat of the air is the ſame with that of temperate of the eighth climate, and

* Fig. 4. † Fig. 1.

the

the weight of the atmoſphere is equal to 28 inches of quickſilver, then you need only looſen the tube of the thermometer from the tube NO, by heating the maſtick as before: but if the quickſilver was not 27 inches above E, the aperture P muſt be cloſed again with maſtick, and the quickſilver poured again by I, till you judge there is ſome air entered into the ball D, ſufficient to ſuſtain the quickſilver in the tube AC, 27 inches above E, which may eaſily be known by the height which the quickſilver will keep in the tube Ll; if on the other ſide the quickſilver in the tube AC ſhould be found the firſt time much above the 27 inches, it would be a ſign that the capacity of the ball M is too large: then the glaſs of the thermometer above the machine muſt be taken away, and emptied, to begin to fill it anew, obſerving, before you put the maſtick again on the tube NO, to put into the ball M a ſufficient quantity of quickſilver, to diminiſh its capacity by about the quantity that may have been thought too great. If there were any perſons who had the muſcles of reſpiration ſtrong enough to reduce the air in D, by blowing thro' A, into the ſame ſtate of condenſation with theſe 27 inches of quickſilver, they need only make the machine ILMNO, and after having introduced a little quickſilver into the ball D with a funnel, blow ſtrongly thro' the aperture A, till the quickſilver could riſe in the tube AC 27 inches above E: but few, if any, are capable of ſo much force, and the ſureſt way is to make uſe of the machine above-mentioned.

In the laſt place, to finiſh the preparation of the thermometer, obſerve with a ſingle barometer, what is then the weight of the atmoſphere, and what height of quickſilver it will ſuſtain:

ſubſtract it from 73 inches, and mark with ſome colour on the tube * CA, beginning over-againſt E, the number of inches and lines remaining from the ſubſtraction. Afterwards dip the ball D in a kettle full of cold water, and ſetting the whole over a pretty good fire, keeping the tube AC always even, leave it till the water boils very briſkly; as faſt as the water heats, you will ſee the quickſilver riſe, ſo that when the water is ready to boil, it will begin to diſgorge by the aperture A, if the weight of the atmoſphere does not then exceed 28 inches of quickſilver; and when it is quite boiling, and there comes out no more quickſilver, it muſt be inclined a little at ſeveral times, to make it come out again, and reduce it to the mark made towards A, that is, to the neceſſary height, to equal with the weight of the atmoſphere a load of 73 inches of quickſilver. Then this thermometer will be finiſhed, and there is no more to do but to take it gradually out of the boiling water, for fear the too great cold of the external air ſhould break the glaſs.

I have obſerved with theſe thermometers, that the air, which we call temperate, ſupports about 19 inches of quickſilver leſs than that which is driven by a degree of heat equal to that of boiling water. I have ſaid, *that which we call temperate*, becauſe we are not ſure that it is true, this knowledge preſuppoſing that of extreme heat and cold, which we do not know yet: but in the mean while, till we can eſtabliſh the neceſſary correſpondences, thoſe who have a mind to know more on this ſubject, may with theſe thermometers make ſeveral experiments to carry their conjectures farther.

* Fig. 1.

Obſer-

Obſervations.

June 16, 1702. I expoſed to the ſun at noon one of thoſe ſpirit of wine thermometers, which being ſet out in the open air, without being always in the ſun, extend 33 inches from the greateſt cold to the greateſt heat experimented at *Paris*. I expoſed near it at the ſame time the new thermometer, which I have juſt deſcribed, and obſerved that the degree of the ſun's heat ſuſtained 13 inches, 2 lines $\frac{1}{3}$ of quickſilver leſs than that of boiling water, and 5 inches, 9 lines $\frac{2}{3}$ more than that of the temperate air of our climate. During the obſervation, there was a ſmall N. E wind, which made the quickſilver ſometimes fall and ſometimes riſe half an inch, whilſt the ſpirit of the other thermometer always roſe in a pretty equal motion; ſo that being arrived to the very top of the glaſs, I was obliged to take it away from the ſun, for fear it ſhould break; the weight of the atmoſphere at that time was equal to about 28 inches of quickſilver. I placed this new thermometer another time in water, where there was a good quantity of ice, and the quickſilver fell but 2 inches below temperate; that is, 21 inches below the degree of heat of boiling water; whence we may probably conjecture, that there ſtill remains in the ice a very conſiderable degree of heat; which we may eaſily know, if we conſider, that after the firſt froſts the common thermometers ſink very conſiderably.

When the quickſilver riſes into the tube BA, the capacity poſſeſſed by the air in the ball D, is greater in ſtrictneſs than when the quickſilver falls from this tube, which it ſhould not be, abſolutely ſpeaking, if the different ſizes of the

balls did not hinder the motion of the quick-ſilver in theſe thermometers from being exactly equal in all. Wherefore in the following experiments, which were made with leſs balls than that already mentioned, and with tubes of a pretty large aperture, which had no proportion to their balls, we muſt not be ſurpriſed if the motion of the quickſilver is not exactly ſuch as has been mentioned; for the inequalities of theſe very experiments have ſhewn the neceſſity of determining more exactly the proportion of the tubes to the balls. We muſt not expect however, that the differences which proceed from the different ſizes of the balls are very conſiderable, much leſs that thoſe differences follow thoſe of the balls; ſince ſuppoſing 2 balls of thermometers, one of 3, and the other of 2 inches in diameter, and that the ball of 3 inches is applied to a tube of an aperture leſs by half than that of the tube applied to the ball of 2 inches, if the quickſilver deſcends in the firſt 19 inches below the place to which the boiling water had made it riſe, it will deſcend in the ſecond at leaſt 18 inches; whereas, according to the proportion of the balls and tubes, it muſt have deſcended only 3 inches in this laſt.

Saturday, July 1, 1702. There were incloſed in two glaſſes of new thermometers 2 unequal maſſes of air, one about double of the other, each of them loaded with 14 inches, 4 lines of quick-ſilver, and beſides with the weight of the atmoſphere, which was found to be 27 inches, 6 lines $\frac{1}{2}$, which made in all 41 inches, 10 lines $\frac{1}{2}$, of which the third part 13 inches, 11 lines $\frac{1}{2}$ was the height to which it was eſtimated that the quickſilver ought to riſe, on the air of the two glaſſes being heated by the heat of boiling water. Theſe meaſures were ſo regulated, the glaſſes dipping in the water,

water, cold as it was then; for by several experiments made afterwards, it has been found, that liquids follow the temperature of the air in which they are. The whole was afterwards set on the fire, which was raised till it made the water boil pretty briskly for a considerable time; and it was observed, that when the quicksilver was risen in the glass with the biggest ball to 13 inches, 1 line; and only to 12 inches, 3 lines in the other, the quicksilver ceased entirely to rise in both: thus the quicksilver rose in the first 10 lines $\frac{1}{2}$ less than was expected; and in the last 20 lines $\frac{1}{2}$. As these experiments were made in haste, and without preparation, the company desiring at first only to see these new thermometers loaded with quicksilver, several circumstances were neglected, which caused these differences.

1. It was not observed whether the state of the cold water, into which the glasses were plunged to regulate them, was that which we here call temperate; for the experiments, which served as a foundation to determine this augmentation of the elasticity of the air to about $\frac{1}{3}$ of its charge were made in this circumstance, it being probable, that this augmentation is more than $\frac{1}{3}$, when the state of the air inclosed in the balls is colder than the temperate, and less than $\frac{1}{3}$ when it is hotter. We had not also used the precaution to have the tubes of a bigness proportioned to the capacity of their balls, because we thought these tubes small enough not to cause any considerable augmentations in the bulks of the air inclosed in these balls.

Wednesday, July 5, 1702. The 2 glasses of the preceding experiment were put again into cold water, but yet hotter than temperate; there was added a spirit of wine thermometer, and an air

one after the new manner, to know by their means the ſtate of this cold water, and ſo make the correction neceſſary to the height of the quickſilver contained in the 2 firſt glaſſes, and it was found:

1. That the ſtate of the water held the ſpirit of wine thermometer at 60 degrees, that is, 10 degrees above temperate, or the ſtate of the air in very deep places, as for inſtance, the caves of the obſervatory.

2. That the air thermometer ſuſtained 15 lines of quickſilver more than the temperate, that is, that the ſurface of the quickſilver in the tube was 27 inches, 3 lines above the ſurface of the quickſilver in the ball.

3. That the ſurface of the quickſilver in the two tubes of the glaſſes of the preceding experiment, was 14 inches, 8 lines above the ſurface of that of their balls.

In the laſt place, it was obſerved on the barometer, that the weight of the atmoſphere was then equal to 27 inches, 5 lines of quickſilver; ſo that to this weight there was added that of 14 inches, 8 lines, which made 42 inches, 1 line, of which the third part 14 inches, and $\frac{1}{3}$ of a line, was the quantity which the quickſilver would have riſen in theſe tubes above 14 inches, 8 lines, if the ſtate of the cold water, in which theſe balls plunged, had been that of temperate: upon which it may be remarked, that tho' the quickſilver of the air thermometer was 15 lines higher than temperate, yet there were taken but 12 lines from the height of the quickſilver of the glaſſes, becauſe the air of their balls not being ſo loaded as that of the ball of the thermometer, it muſt not have increaſed its ſpring ſo conſiderably: ſo that it was determined, that the

quick-

quickſilver in the two glaſſes of the experiments muſt have riſen only 13 inches, $\frac{1}{3}$ of a line, by the heat of the boiling water.

As it was wanted only to know by experience, whether unequal maſſes of air, equally loaded, increaſed the force of their ſpring equally by the ſame degree of heat; but alſo to know, whether this augmentation was ſo much the greater, as theſe maſſes were the more loaded, and whether it was always about $\frac{1}{3}$ of the loads of the air in the ſtate of temperate: to be certain of this by the ſame experiment, it was determined alſo to what height the quickſilver ought to riſe in the air thermometer, by the heat of the boiling water; and as the height, at which it was then found in the cold water, was 27 inches, 3 lines, which being added to 27 inches, 5 lines, the weight of the atmoſphere at the time of the experiment, made in all 54 inches, 8 lines; of which the third part 18 inches, 2 lines $\frac{2}{3}$, was the quantity which it ought to have riſen by the heat of the boiling water: but as the cold water, tho' warmer than temperate, in which it was dipped, kept it up 15 lines above temperate, it was determined that the quickſilver could not riſe again more than 16 inches, 11 lines $\frac{2}{3}$, by the heat of the boiling water.

After having thus determined on theſe three glaſſes the height to which the quickſilver ought to riſe, namely, in the 2 firſt to 13 inches, $\frac{1}{3}$ of a line, and in the laſt to 16 inches, 11 lines $\frac{2}{3}$; the whole was ſet on the fire as before, till the water quite boiled; and the quickſilver roſe to the height that it ought in that of the two firſt glaſſes, where the ball was biggeſt, and where the difference was but 10 lines $\frac{1}{2}$ in the preceding experiment: but in the ſecond, there wanted about

6

6 lines; and in the thermometer, or third glaſs, there wanted 2 lines $\frac{2}{3}$, which, without doubt, proceeded from the bigneſs of their tubes of three glaſſes, being conſiderably diſproportioned to the bigneſs of their balls, and from the bulks of air not remaining conſtantly the ſame, but altering as the quickſilver of the balls is driven into the tubes, as was ſaid before; for tho' it is proved by all theſe experiments, that the air does not dilate in proportion to its bulk, as ſpirit of wine, and all other liquors do, and that therefore it does not ſeem neceſſary, that the balls and their tubes ſhould be ſo proportioned to each other, as to acquire equal degrees of elaſticity, the bulks of air muſt remain the ſame, or at leaſt increaſe proportionably from what they were before the heat had acted upon them; and tho' it is impoſſible, let the tubes be ever ſo narrow, that the quickſilver driven in ſhould not alter theſe bulks a little; yet it is neceſſary, in order to obtain a perfect uniformity in the motion of the quickſilver of theſe thermometers; that the tubes ſhould be nearly proportioned to their balls; for the little exactneſs is here of no conſequence.

Saturday, July 8, 1702. A glaſs of the new thermometer was charged with quickſilver, after the ſame manner, and with the machine deſcribed in the former part of this paper. The glaſs was afterwards put in water over the fire, which was increaſed as uſual till it quite boiled. In this ſtate we finiſhed the reducing of the height of the quickſilver, which was riſen higher than the 45 inches above that of the ball exactly at theſe 45 inches as was ſaid before, except that there was no regard had to the weight of the atmoſphere, which was then 27 inches, 4 lines, that is, 8 lines lighter than it ought to have been, and that conſequently there

there muſt have been 45 inches, 8 lines of ſurface to the other to make the total charge be 73 inches.

I have ſaid before, that the air which we call temperate, ſuſtained 19 inches of quickſilver leſs than the heat of boiling water. The day before theſe experiments were made, I placed two of theſe new thermometers in the caves of the obſervatory, one fell to 18 inches, 10 or 11 lines, the other only to 18 inches, 6 or 7 lines. From all theſe experiments therefore we may conclude,

1. That when the bigneſs of the tubes is not proportioned to the capacity of the balls, unequal maſſes of air increaſe almoſt equally the force of their ſpring by the ſame degree of heat.

2. That the more theſe maſſes of air are loaded, the more they augment the force of their ſpring by the ſame degree of heat.

That this augmentation would probably be about $\frac{1}{3}$ of the loads at the time of temperate, if theſe maſſes did not increaſe their bulks, by driving into the tubes a part of the quickſilver contained in the balls.

4. And that laſtly, it is probable alſo, that the effects would be uniform in all theſe glaſſes, of what ſizes ſoever the balls may be, if the capacities of theſe balls were proportioned to the bigneſs of their tubes, as I have really found by experiment; upon which it may not be amiſs to obſerve, that having broken the glaſſes uſed in the experiments of *July* 5, in which the quickſilver ought to have riſen to 13 inches, $\frac{1}{3}$ of a line by the heat of boiling water, tho' it did not riſe to this height, except in the firſt of the two glaſſes, and only to 12 inches, 6 lines $\frac{1}{3}$ in the ſecond; and that having exactly meaſured with quickſilver the capacity both of the tubes and the

balls, I found that on the length of 31 inches the capacity of the firſt tube was $\frac{1}{67}$ part of the capacity of its ball, and that the capacity of the tube wherein the quickſilver roſe only to 12 inches, 6 lines $\frac{1}{3}$, was $\frac{1}{39}$ part of the capacity of its ball; whence we ſee, that tho' this laſt tube was almoſt twice as big as it ought to have been, yet the difference was but 6 lines, that is, about $\frac{1}{26}$ part of the height to which the quickſilver roſe, whereas it ſhould have been near half, that is, about 6 inches, if the motion of the quickſilver in theſe two glaſſes had been made according to the proportion of the tubes to the balls, as it would have happened, if they had been full of ſpirit of wine, or any other fluid than air. We ſee alſo by this experiment, that the ſmaller the capacity of the tubes is, in compariſon of the balls, the more the increaſe of the ſpring of the air, by the heat of the boiling water above what it is in the temperate ſtate, approaches more truly to the third part of the load which this air ſupports: but as theſe tubes were already ſo ſmall, that it is not convenient to leſſen them, it would be better to increaſe the bigneſs of the balls, and to make them 3 or 4 inches in diameter.

M. Mariotte's *rule for the* equilibrium *of the air by its ſpring.*

When the height of the quickſilver with which we propoſe to load a maſs of air, preſſed at firſt only by the weight of the atmoſphere, which he ſuppoſes with me equal to 28 inches of quickſilver, is given, and we would find the bulk to which the air would be reduced by this load, M. *Mariotte* conſiders this maſs of air as incloſed in the branch * EC, of the tube ABC of equal

* Plate XVI. Fig. 5.

bigneſs

bigneſs in its whole length, open at A, bent ſquare at D and E, and cloſed in C, the part B is full of quickſilver to the prickt line DE, the branch DA ſerving to contain the loads which ſerve to preſs the air at EC ; after this, M. *Mariotte* makes the following analogy : As the ſum of the weight of the atmoſphere, and of the height of the quickſilver, with which we propoſe to load the maſs of air EC is to 28 inches of the atmoſphere, ſo is the bulk of air EC to the bulk to which this load reduces it.

Now to make application of this rule to our experiments, let us ſuppoſe 3 tubes, as ABC, in which the tubes EC are to one another in the proportion of 1, 2, 3, and conſequently the maſſes of air which they incloſe : let us ſuppoſe alſo the load, with which theſe maſſes of air are to be preſſed, equal to 45 inches in height of quickſilver ; then to have the bulk to which the air will be reduced in the firſt glaſs, it muſt be, 1. as 73 inches (the ſum of the weight of the atmoſphere 28 inches and of the load 45 inches) is to 28 inches (the weight of the atmoſphere) ſo is 1 (the bulk of air preſſed only by the atmoſphere) to $\frac{28}{73}$ the bulk of air loaded with 45 inches in this firſt glaſs. 2. To have the bulk to which the quickſilver will be reduced in the ſecond glaſs, it muſt be as 73 inches to 28, ſo 2 to $\frac{56}{73}$ the bulk of air loaded with 45 inches in the ſecond glaſs. 3. In the laſt place, to have the bulk to which the quickſilver will be reduced in the third glaſs, it muſt be as 73 inches to 28 inches, ſo 3 to $\frac{84}{73}$ the bulk of air loaded with 45 inches in the third glaſs. Now as theſe fractions $\frac{28}{73}$ $\frac{56}{73}$ $\frac{84}{73}$ are to one another as the numbers 1, 2, 3, theſe unequal maſſes of air by acquiring equal forces of elaſticity, have not changed the proportion

that they had to each other, and conſequently they muſt, continuing the ſame, acquire equal force of elaſticity, ſeeing the cauſe which produces them is equal, as the degree of heat is here ſuppoſed to be.

Beſides, we can hardly have any other idea of the parts of fire, than that they are in a continual and very violent motion; nor can we conceive how theſe parts can heat thoſe of the moſt ſolid bodies, but by ſuppoſing that by the effort which they make to penetrate them, they communicate a part of their motion to them.

But as in the experiments which ſhew that unequal maſſes of air acquire equal forces of elaſticity by the ſame degree of heat, it is eaſy to judge by the preceding calculation, that all the parts of air which compoſe the three different bulks of air, are neither more nor leſs cloſe the one than the other, and that beſides, the parts of fire, which put them into motion, being in like manner the ſame, they cannot communicate more to one than to another. It is true, that unequal maſſes of air cannot acquire unequal forces of elaſticity by the ſame degree of heat, but on the contrary, they muſt acquire equal ones, and this is what is confirmed by experience.

As for the ſame maſſes acquiring ſo much greater forces of elaſticity, by the ſame degree of heat, as they are more loaded, it is eaſy to conceive, that the more the maſſes of air are loaded, the more parts of air they contain in the ſame ſpace, and conſequently the parts of fire cannot inſinuate themſelves between theſe parts of air, with the violence which we know they employ in ſeparating the moſt immoveable parts of the moſt ſolid bodies, without ſeparating theſe parts of air from each other; whence it neceſſarily follows,

that the more parts of air there are in the ſame ſpace, the greater the augmentation of the bulk, to which the heat reduces it, ought to be: but as the cauſe, which would augment the bulk of an elaſtic body, ſuch as the air is if it had liberty to extend itſelf, would in like manner augment the force of its ſpring if it had not this liberty; it neceſſarily follows, that the more the maſſes of air are loaded, the more the ſame degree of heat muſt make them acquire a greater elaſtic force, this is what really does happen.

As for the experiment ſhewing that the elaſtic force, which the air acquires, when it is heated by the warmth of boiling water, is about $\frac{1}{2}$ of what it has been when temperate; we do not yet ſufficiently know whether this happens by a neceſſary conſequence of ſome principles, or whether it is a mere effect of chance. In the mean time all that we can do therein, is to aſſure ourſelves, by a long ſeries of experiments, of the truth of the fact.

III. *A compariſon of the ancient itinerary meaſures with the modern, by M.* Caſſini *.

As the deſcription of all the earth is made by the dimenſions, that have been taken in diverſe places and at diverſe times, as well in the heavens as in the earth, and as the meaſure of the earth is determined variouſly by various people, and changes with time; nothing is of more conſequence in geography, than to know the proportion of the itinerary meaſures, which the ancient geographers uſed in the deſcription of a country to the modern meaſures.

The itinerary meaſures are ſometimes different

* Jan. 28, 1702.

from

from thoſe, which are uſed in commerce and in architecture. We fall into great errors, when we uſe them indifferently in geography.

The meaſure of the diſtance from Narbonne *to* Niſmes.

In the laſt journey that we made by the king's order into ſeveral provinces of *France*, we have compared the diſtances that we have found bebetween the ancient towns and thoſe of the ſame towns, related by the ancient geographers. We ſhall here relate ſome examples of them. The diſtance from *Narbonne* to *Niſmes*, by our dimenſions, is 67500 *Paris* toiſes.

Strabo places *Narbonne* 88 miles from *Niſmes*; the road from one of theſe towns to the other is pretty ſtraight, and there is but little reduction to make. Divide 67500 toiſes by 88 miles, there comes to each 767 $\frac{1}{22}$. We throw out this ſmall fraction, becauſe we cannot pretend to have exactly the ſame bounds of theſe two towns, which were taken by the ancients. Each pace was 5 feet, and the mile 5000 feet, the foot is divided into 12 inches. The toiſe is 6 *Paris* feet, therefore 767 toiſes make 4602 feet. Rejecting two feet in ſo large a number, of which it is difficult to be certain in the practice, in round numbers 4600 *Paris* feet will be equal to 5000 ancient geographical feet, which are as 46 to 50, or 23 to 25.

Thus the *Paris* foot of 12 inches will be equal to a foot and an inch and $\frac{1}{23}$ of an inch of the ancient meaſure, and the ancient foot will be equal to 11 inches and $\frac{1}{25}$ of the *Paris* foot. If we ſuppoſe the ancient mile be 764 toiſes it will be ſmaller by 3 toiſes than by this compariſon, and the ancient geographical foot will be exactly

to the *Paris* foot, as 11 to 12. We muſt now ſee if the other ancient geographers agree with *Strabo* in this meaſure.

By the itinerary of *Antoninus*, they reckon one time between *Niſmes* and *Narbonne* 87 miles, another time 91; the dimenſion of *Strabo* is between the two. By the ancient table of *Peutinger*, they reckon it 95. We ſhall prefer the dimenſions of *Strabo*, who lived in the time of *Auguſtus* and *Tiberius*, the meaſures of the great roads having then been made with care. We have nevertheleſs examined which of theſe meaſures agree moſt with others, which have been taken in *Italy*, as well at the time of the *Romans*, as in our time.

The meaſure of the diſtance from Bolonia *to* Modena.

Antoninus's itinerary marks many times the diſtance from *Bolonia* to *Modena*, and always makes it 25 miles. The table of *Peutinger* makes it alſo 25 miles.

Theſe two cities are croſſed by the *Emilian* way, which in this interval was ſtraight. The fort *Urbano*, which they have built upon it, makes it at preſent turn a little. But we ſhall make uſe of the ſame, which has been taken by the means of triangles in a right line.

F. *Riccioli* and *Grimaldi* have carefully taken the diſtance between the towers, which are in the middle of theſe two cities of a great height. I aſſiſted at ſome of the obſervations that they made at *Bolonia*, and I examined their ſtations at *Modena*. They found the diſtance between theſe two towers 19,666 paces of *Bolonia*, which are 5 feet each. The *Bolonian* foot drawn from the ſame original, from whence F. *Riccioli* took his, compared to the *Paris* foot by us, is to the *Paris* foot,

foot, as 701 to 600. Thus 600 feet of *Bolonia* are equal to 701 *Paris* feet. The pace of *Bolonia* is 5 *Bolonian* feet; and the *Paris* toiſe is 6 *Paris* feet: divide 600 feet by 5, we ſhall have 120; and multiply that by 6, and we ſhall have 720 paces of *Bolonia*, equal to 701 toiſes of *Paris*.

Now, as 720 is to 701, ſo is 19,666 paces of *Bolonia* to 19,147 toiſes of *Paris*, which is the diſtance from *Bolonia* to *Modena*, by the dimenſion of *F. Riccioli* and *Grimaldi* reduced into toiſes. But this diſtance, by the agreement of the ancient itineraries, is 25 ancient miles: dividing then 19,147 toiſes by 25 miles, we ſhall have 766 toiſes for a mile, within almoſt one toiſe of 767, which we found before, by the compariſon of the diſtance between *Niſmes* and *Narbonne* given by *Strabo*, with thoſe which we have determined by our obſervations.

An inquiry into the ſituation of the temple of the Pyrenean Venus.

We made uſe of this meaſure of the ancient miles to find the place where the temple of the *Pyrenean Venus* formerly was, which *Strabo* places on the confine of the *Narbonneſe Gaul* with *Spain*, 63 miles diſtant from *Narbonne*. This diſtance at the rate of 767 toiſes for a mile, following the dimenſions drawn from that of *Narbonne* to *Niſmes*, would be 48,321 toiſes. Altho' the etymology ſhews, that *Port-Vendre* is the port of *Venus*, as *Vendredi* is the day of *Venus*, yet this diſtance does not agree at all with that of *Port-Vendre*, which is near *Colioure*. Perhaps the port of *Venus* was diſtant from the temple of *Venus*, or there were two ports on that ſide a little diſtant from one another, which had the ſame name.

name. There was another *Port-Vendre*, called now *l'Etang de Vendre* near *Narbonne*. The diſtance that is between *Port Vendre*, which is near *Colioure* and *Narbonne*, according to our dimenſions, is 41,000 toiſes leſs than that which we have juſt found by 7,321 toiſes.

At the diſtance of 48,300 toiſes from *Narbonne* is the *Selve*, where there is a port capable of a good number of gallies, with a tower to defend the road. It is larger than *Port-Vendre* near *Colioure*, and is ſituated on the N. ſide of *Cape Creux* which is the celebrated *Aphroditic* promontory, which *Strabo* alſo calls the *Pyrenean* promontory.

Mela ſays, that between the promontories formed by the *Pyreneans*, there is the port of of *Venus*, celebrated on account of the temple; which might be underſtood as well of one as the other port. *Pliny* places the temple of the *Pyrenean Venus* XL miles diſtant from the river *Tichis*, now *Ter*, in *Catalonia*. This diſtance is without doubt too great, and almoſt double of that which comes from the preceding compariſon.

M. *de Marca*, inſtead of XL, reads XI, ſuppoſing that the I has been changed into L, but this diſtance is too ſmall, and does not agree even with the diſtance between the *Tech* and *Port-Vendre*, near *Colioure*, which is but 5500 toiſes, a little above 7 miles. Probably, inſtead of XL miles it ſhould be read XX miles; and the laſt X has been changed to an L. Thus the port of *Venus* will be the port of *la Selva*.

The meaſure of the ſtadia in France.

Strabo places the diſtance between the temple of the *Pyrenean Venus*, and the outlet of the *Var*, which he gives for the two bounds of *France*, at 277 miles. He ſays, that to this diſtance is reckoned

2600 *ſtadia*; and that others add alſo to it 200 *ſtadia*, which would make in all 2800 *ſtadia*.

We ſee by this, how different the proportion of the miles is to the *ſtadia*. In dividing theſe two numbers of *ſtadia* by 277 miles, the firſt number gives 9 *ſtadia*, and a little more than $\frac{1}{3}$ for a mile; and the ſecond, 10 *ſtadia* and above $\frac{1}{9}$ for a mile. Altho' in other places, *Strabo*, and the reſt, commonly give but 8 *ſtadia* to a mile; this compariſon however ſhews, that we cannot here allow leſs than 9 *ſtadia* to a mile, divide 765 toiſes, which make an ancient mile, by 9, we ſhall have a *ſtadium* in *France* of 85 toiſes, which make 510 *Paris* feet. *Herodotus* makes the *ſtadia* 600 feet, the foot of *Herodotus* will then be to the *Paris* foot, as 51 to 60, ſuppoſing the *ſtadium* of *Herodotus* equal to the *ſtadium* of *France*.

The meaſure of the pyramids of Egypt *in feet and* ſtadia.

Herodotus makes the breadth of the baſe of the greateſt pyramid of *Egypt* to be 800 feet, and conſequently one *ſtadium* and $\frac{1}{3}$, and as 60 is to 51, ſo 800 is to 680 *Paris* feet, for the breadth of the baſe of the pyramid. At the rate of 9 *ſtadia* for a mile, of which each has 510 feet, this baſe would have a *ſtadium* and $\frac{1}{3}$, as by the dimenſion of *Herodotus*. M. *Chazelles* has actually meaſured the baſe of this pyramid by a line, and found it 690 feet, upon an unequal ground raiſed in the middle; from whence he ſays, there muſt be ſomething taken away to have the juſt baſe. If we take away 10 feet, we ſhall have for the breadth of the baſe 680 *Paris* feet, as we have before calculated.

M.

M. *Gemelli*, who lately went round the world, relates the measure of this pyramid, which he saw in 1693, as he had them from F. *Fulgence* of *Tours*, a mathematical capucin, who found the breadth of this pyramid on each side to be 682 *Paris* feet, which agrees with the measure that we have just found at the rate of 9 *stadia* for a mile. The measures which he gives of it agree with those that M. *Jeaugeon* has had from M. *de Nointel*, the king's ambassador to the *Porte*, and which he has communicated to the academy. It is surprizing, that Mr. *Greaves*, an *English* mathematician, in his *Pyramidographia*, has found the base of this great pyramid measured, by triangles, to be 683 *English* feet, which are to the *Paris* feet, as 15 to 16. In this proportion, having supposed the breadth of the pyramid 680 *Paris* feet, it must be 723 *English* feet, from whence we may see the difference, that there is between the measures of the same magnitude, taken by diverse persons, and reduced to the same feet.

Strabo himself, whose measures taken in *France*, we have compared with our own, and who was in *Egypt* with *Ælius Gallus* about the christian *epoch*, makes the breadth of this pyramid one *stadium*. He therefore makes the *stadium* here $\frac{1}{5}$ greater than *Herodotus*, or the geographers, from whom he has drawn the dimensions of the southern coasts of *France*.

Diodorus Siculus, who was in *Egypt* 60 years before the christian epoch, says, that the greatest pyramid had on each side of its lower part 7 *jugera*; 6 *jugera* make a *stadium* according to *Herodotus*: therefore each side of the base of the pyramid was one *stadium* and $\frac{1}{6}$. We have therefore three different dimensions of the pyramid in *stadia*; one of an exact *stadium*, one of a *sta-*

dium and $\frac{1}{6}$, and one of a *ſtadium* $\frac{1}{2}$. The meaſure of the *ſtadia* was therefore as different and as equivocal among the ancients, as the meaſure of the miles and leagues are among the moderns. The meaſure of miles was more uniform, as we have found by the compariſon of the ſame diſtances taken in *France* and *Italy*, by the ancients and by the moderns. We have from this compariſon drawn a concluſion of no ſmall importance, which is, that the modern *Roman* foot of a palm and $\frac{1}{3}$, is equal to the ancient foot uſed in the meaſure of the diſtances of the towns of *France*, and that both are to the *Paris* foot, as 11 to 12, having neglected a ſmall fraction, which is inſenſible in the practice.

But the foot of *Herodotus*, with which he meaſured the pyramid, being to the *Paris* foot, as 51 as 60 is equal to 10 inches, 2 lines and $\frac{2}{5}$ of the *Paris* foot. It is the large foot of a man of tall ſtature, and ſuch muſt be the foot of *Hercules*, with which he meaſured the *ſtadia* for the *Olympic* games, allowing them 600 of his feet, which made 100 paces according to *Herodotus*. This author divides the pace into 6 feet, as we divide the toiſe into 6 feet. It is probable, that *Eratoſthenes*, who allowed 700 *ſtadia* to a degree of the circumference of the earth, having drawn it from the diſtance between *Alexandria* and *Syene*, made uſe of theſe *ſtadia* of *Herodotus*. Thus a degree, according to *Eratoſthenes*, would be the product of 85 toiſes by 700, which makes 58,500 toiſes. This meaſure of a degree is greater than ours by about $\frac{1}{44}$ part.

Pliny allows 883 feet to the length of each ſide of the baſe of the largeſt pyramid. Theſe are not the feet of the itinerary meaſure, that we have found by many compariſons to be to the *Paris* foot,

foot, as 11 to 12. For in this proportion the baſe, which has been found to be 780 *Paris* feet, ought to be 702 feet of the ancient itinerary meaſure, inſtead of 883 that *Pliny* allows them. There is then 181 feet difference, which makes more than $\frac{1}{4}$ of 702. This meaſure is therefore to the ancient itinerary foot, which we found before to be equal to the modern *Roman* foot, as 12 to 15, and a little more, and exceeds the modern *Roman* palm only $1\frac{1}{5}$, which is to the *Roman* foot, as 12 to 16. It is therefore probable, that *Pliny*'s was a foot of architecture of different meaſure, from the *Roman* foot and palm.

There is alſo a more conſiderable difference in the meaſure of the ſquare place, which remains at the top of this pyramid. *Pliny* makes its breadth 25 feet; *Gemelli* relates it to be 16 feet $\frac{2}{3}$. In proportion to the meaſure of the baſe, as 682, the meaſure of *Gemelli* is to 883 the meaſure of *Pliny*, ſo 16 feet $\frac{2}{3}$ are to 21 feet $\frac{1}{3}$, inſtead of 25 that *Pliny* gives us. There is 3 feet $\frac{1}{3}$ difference, we may attribute it to the deſtroying of the cruſt of marble with which this pyramid muſt have been covered, in the time of *Pliny*, as the other pyramids, one of which remains at preſent covered at the point, the reſt having been demoliſhed. The thickneſs of this cruſt muſt have been a foot and $\frac{2}{3}$ of *Pliny*'s meaſure. This diminution of the baſe, which has happened ſince *Pliny*'s time, does not ſenſibly vary the proportion of diverſe feet that we have examined, and does not ſettle the different dimenſions that are given.

If it is ſo difficult to make the meaſures of the ſame baſe agree, which ſubſiſt always without ſenſible variation, and which we may meaſure exactly without difficulty, we may judge how difficult it is to be aſſured of the diſtances of

towns

towns which have not actually been measured, but have been determined by the gross estimate of the time commonly spent in passing from one to the other. We must nevertheless have the distances from one place to two others, of which the situation is known to determine with respect to them the position of a third by triangles. Inevitable errors multiply according to the multitude of places, and there remains no better way of correcting them, than by the observations of stars made in places very distant from one another.

The measure used by pilots.

The pilots of the *Mediterranean* allow 75 miles to a degree. Those of the ocean allow only 60. The ancient *Italian* miles are to the modern miles as 60 to 75; for the ancients make the distance from *Bolonia* to *Modena* 25 miles; and the moderns reckon only 20 miles from one of these towns to the other. Therefore those of the *Mediterranean* make use of the ancient miles, which are to this day in use in several provinces of *Italy*; and those of the ocean make use of the modern miles, which are in use in other provinces. The modern measure has this convenience, that it takes a minute for a mile; whereas the ancient allows to each minute a mile and a quarter. We may accommodate ourselves to the use of both. If we give 5 feet to the ancient pace, as they do in *Italy*, a degree of 75000 paces will be 375000 feet; and supposing a degree of the circumference of the earth 343,000 *Paris* feet, as we have found it pretty near, this ancient *Italian* foot would be to the *Paris* foot, as 343 to 375, or as 11 to 12 $\frac{1}{39}$; and if we give to the modern *Italian* pace 6 feet, the degree of 60 miles will be 360,000 feet, the modern *Italian* mile of 1 minute, will be 6000 feet, which will be to the

Paris

Paris foot, as 344 to 360, or as 44 to 45. If there are more or leſs *Paris* feet in a degree, the proportion of the *Paris* foot to the *Italian* foot will be a little different, unleſs there happens ſome variation in the number of the ancient or modern *Italian* feet in a degree. For we obtain it, as the ſeamen do from the diviſion of the degree, by the approximation of theſe meaſures to thoſe of ſome country of *Italy*, from whence they have taken the name; altho' the feet, which we call modern, approach more to the common feet of *France*, than to thoſe which are in uſe in the greateſt part of the towns of *Italy*. We allow 6 of them to a pace, as *Herodotus* has done, contrary to the ancient and modern cuſtom of *Italy*, by this means bringing it nearer to the *Paris* foot, and imitating the diviſion of the toiſe into 6 feet, having ſeen that the pace of *Bolonia* is much nearer to the *Paris* toiſe, than the *Paris* foot is to the *Bolonian* foot. By this means a minute of a thouſand paces has 6000 feet; a ſecond has 100 feet, as a degree of 60 minutes has 60,000 toiſes, which are very convenient numbers to uſe, and eaſy to be calculated.

Of the trigonometrical meaſure.

It muſt be obſerved, that in the trigonometrical table, where the ſemi-diameter of the circle is ſuppoſed to be divided into 10 millions of parts, a minute as well as its ſine and tangent, which do not ſenſibly differ in ſo ſmall an arch, is marked with 2909 parts. Doubling the *radius* and the arch, we ſhall have the ſemi-diameter of 20 million of parts, a minute of 5818 parts. But a minute is 6000 geometrical feet, and 5818 is to 6000, as 32 to 33. We may then eſtabliſh a trigonometrical foot, which ſhall be to the geometrical or modern *Italian* foot, as 33 to 32.

We

We may find the proportion of this foot to any other, when we have found how many other feet make a minute of a great circle of the earth. We may, in fine, eſtabliſh a fathom of 2 trigonometrical feet, of which there will be 10 millions in the ſemi-diameter of the earth; thus all the numbers of the table will be ſo many trigonometrical fathoms of two feet, of which there are 2909 in a minute, and 48 ½ in a ſecond, as we ſee without calculating at the head of the table. The third part of the numbers of the table will be ſo many trigonometrical toiſes, of which there are 970 in a minute, and 16 in a ſecond.

Theſe meaſures of geometrical and trigonometrical feet and fathoms, are means between different feet and fathoms, which are eſtabliſhed by different nations. We may then take them for univerſal invariable meaſures. Thus, if it is aſked how many miles, feet, or toiſes are in a determinate arch of the circumference of the earth, we need only take the number of minutes contained in the arch propoſed for the number of geometrical miles, multiply them by 1000 to have the number of geometrical paces or toiſes, or by 60,000 to have the number of feet; thus a degree of 60 minutes will be 60,000 toiſes. The whole circumference of the earth, which is 360 degrees will therefore be 21,600,000 geometrical toiſes, or 21,600 *Italian* miles; and becauſe the circumference is to the ſemi-diameter as 44 to 7, or as 220 to 35, or 21,600 to 3,436, the ſemi-diameter of the earth will be 3436 geometrical miles or modern *Italian* ones. The half 1718 will be the number of geometrical leagues almoſt equal to the ſmall league of *France*, as thoſe which they reckon from *Paris* to *Orleans*. We may take a third for the mean ones which

approach to thoſe of *Auvergne*, and a quarter for the greateſt which approach to thoſe of *Languedoc*.

As for the trigonometrical meaſures, the ſemi-diameter of the earth being ſuppoſed of 10,000,000 trigonometrical fathom, the circumference will be 62,831,852 fathoms.

The third part of theſe numbers will give the trigonometrical toiſes. The ſemi-diameter of the earth will then be 3,333,333 trigonometrical toiſes, and the circumferences will be 20,943,950 trigonometrical toiſes. The $\frac{1}{1000}$ part of theſe two numbers will give the trigonometrical miles.

The ſemi-diameter of the earth will then be 3333 trigonometrical miles, and the circumference 20,944 trigonometrical miles.

Of the horary meaſures.

I have tried ſeveral times in going to *Fontainbleau*, and back again a good pace in a coach, that in the plain of *Longboyau*, which has been meaſured exactly, one travels 5 minutes of the circumference of the earth in an hour. A man on foot would go $\frac{1}{2}$ this way in the ſame time, and a degree in 24 hours; and if he was to travel 12 hours in a day, in a like road, with the ſame quickneſs, he would go round the world in two years.

IV. *Reflections on the meaſure of the earth, related by* Snellius, *in his book intitled,* Eratoſthenes Batavus, *by M.* Caſſini *the ſon**.

Snellius † meaſured on a plain near *Leyden*, a baſe of 326 *Rhinland* perches and 4 feet: (the

* March 1, 1702. † Plate XVI. Fig. 6.

perch contains 12 feet, and the proportion of the *Paris* foot to the *Rhinland* foot is, according to M. *Picard*, as 1440 to 1390) he took from the extremities of this baſe, with a ſemi-circle of 3 feet ½, the angles with the towers of *Leyden* and *Soeterwoude*, and he determined their diſtance to be 1092 perches. By this diſtance, he determined the ſituation of the greateſt part of the towns in *Holland*, and of ſome in *Flanders* by trigonometry. The two towns where he obſerved, which are moſt to the N. and S. are thoſe of *Alkmaer* and *Bergen op Zoom*. He transferred to *Alkmaer* a meridian line, that he had drawn at *Leyden*, and having found the angle that the line drawn from *Alkmaer* to *Bergen op Zoom* made with the meridian, he determined the part of the meridian intercepted between the parallels of theſe towns to be 34,018 perches.

He afterwards obſerved with a quadrant of 5 feet ½ *radius*, the height of the pole at both theſe towns. He found that of *Alkmaer* to be 52° 40′ ½, and that of *Bergen op Zoom* 51° 29′, which is leſs than the preceding by 1° 11′ ½, and having ſubſtracted 25 perches of the diſtance between *Alkmaer* and *Bergen op Zoom*, for the reduction of the places where he had made his obſervations, he determined the magnitude of the degree of the circumference of the earth to be 28,473 perches. Having alſo obſerved the height of the pole at *Leyden*, he determined by the arch of the meridian intercepted between theſe two towns, the magnitude of the degree to be 28,510 perches. Taking a mean between theſe two determinations, we have the magnitude of the degree 28500 *Rhinland* perches, or 55,021 *Paris* toiſes.

Snellius's

Snellius's method is the ſame that we uſed in the prolongation of the meridian. The baſe which he actually meaſured, appears however very ſmall, which might have occaſioned conſiderable errors in the ſeries of his triangles: but as he has verified his meaſures by a new baſe, pretty near the ſame magnitude, we ſhall ſuppoſe his obſervations to be ſuch as he relates, and we ſhall ſee hereafter what reſults from them, being compared with the heights of the pole, that I have obſerved in ſome towns in *Holland*, which are contained in his triangles.

During my ſtay in *Holland*, whither I had carried an octant of 3 feet $\frac{1}{2}$ *radius*, which we have ſince uſed in our laſt journey, I obſerved, the 10th of *Nov.* 1697, the meridian height of the polar ſtar to be 54° 16′ 5″ at *Rotterdam*, which is one of the moſt ſouthern cities of this province. Going afterward to *Alkmaer*, which is the capital of *North Holland*, I obſerved the meridian height of the polar ſtar to be 54° 58′ 10″. The difference between theſe heights is 42′ 5″, which is the arch of the meridian intercepted between the parallels of *Alkmaer* and *Rotterdam*, neglecting the difference of refraction, which does not amount to 2″. The buſineſs is then to know how many toiſes this arch of the meridian is, that we may afterwards determine in toiſes the magnitude of the degree of a great circle of the circumference of the earth. We may thus draw it from the obſervations of *Snellius*.

Snelluis determines the difference of the arch of the meridian intercepted between the parallels of *Alkmaer* and *Leyden* to be 14,215 perches. From *Leyden* he obſerved, that the tower of *Goude* declines from the meridian 44° 49′ towards the E. And he determined in the ſeries of his triangles,

the angle between *Goude* and *Rotterdam* of 43° 36′, by which *Rotterdam* is more to the W. The tower of *Rotterdam* declines therefore from the meridian of *Leyden* 1° 13′ toward the eaſt, and the diſtance between *Leyden* and *Rotterdam* being, according to *Snellius*, 6972 perches, we ſhall have the arch of the meridian between *Leyden* and *Rotterdam* 6970 perches, which being added to the diſtance of *Alkmaer* and *Leyden* upon the meridian, of 14,215 perches, give the diſtance between *Alkmaer* and *Rotterdam* of 21,185 *Rhinland* perches, which being reduced to *Paris* feet, make 40,913 toiſes. The place where I obſerved at *Alkmaer*, drawn from the plan of this town, is 20 or 30 toiſes more to the S. than the great church where *Snellius* obſerved; and the place where I obſerved at *Rotterdam*, is 30 or 40 toiſes more to the N. than the tower of the great church, which is probably that where *Snellius* obſerved, ſince it is diſtinguiſhed from all the reſt by its height, as I have obſerved in my journal.

The places where *Snellius* obſerved being therefore one more northward, and the other more ſouthward than thoſe of my obſervations, by adding their difference, we ſhall have 60 toiſes, that muſt be retrenched from the diſtance between *Alkmaer* and *Rotterdam*, and we ſhall have in the interval 42′ 5″, which have been obſerved between theſe two towns 40,833 *Paris* toiſes, and 58,245 toiſes for a degree. This meaſure exceeds that which we have determined by the obſervations made in the laſt journey, by more than 1000 toiſes, pretty near agreeing to that which *Snellius* determined by his obſervations.

This difference appeared ſo conſiderable to me, that I thought myſelf obliged to examine the me-

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thod which *Snellius* uſed, and calculate his triangles upon the obſervations that he has made, and related in his book. I have been alſo led to it by ſome errors of the preſs, which immediately ſtrike the eye, and among others, *lib.* II. *p.* 173. *l.* 20 and 21. inſtead of *diſtantia inter Leydam & Rotterodamum*, we ſhould read *inter Goudam & Rotterodamum*, I have therefore firſt calculated upon his baſe actually meaſured the diſtance between *Leyden* and *Soeterwoude*, which agrees with what he has marked. Upon this diſtance, I have calculated that between the *Hague* and *Leyden*, which I have found, as well as he, to be 4103 perches, and in the triangle AEF made by the *Hague*, *Leyden*, and *Rotterdam*, the diſtance AE, between the *Hague* and *Leyden*, being already known, the angle AFE, which *Leyden* and the *Hague* make with *Rotterdam*, being obſerved to be $39^{\circ}\ 53''$, and the angle AEF, that *Rotterdam* and the *Hague* make with *Leyden*, being obſerved to be $53^{\circ}\ 40'$, I found the diſtance EF, between *Leyden* and *Rotterdam*, 6386 *Rhinland* perches, and the diſtance AF from the *Hague* to *Rotterdam* 5154. *Snellius*, *lib.* II. *p.* 173. *l.* 12. in the ſame triangle, where he relates the angles obſerved which I uſed, gives the diſtance AF between the *Hague* and *Rotterdam* of 5616, and the diſtance EF from *Leyden* to *Rotterdam* 6972, greater than that which reſults from the calculation by 586 *Rhinland* perches, which make more than 1100 toiſes.

Suppoſing the diſtance AE between the *Hague* and *Leyden* 4103. He afterwards determines in the triangle AES made by the *Hague*, *Leyden*, and *Goude*, the diſtance ES from *Leyden* to *Goude* 5898 perches; and having obſerved from the two extremities of this baſe, the angles at

Rotterdam,

Rotterdam, he determines in another triangle EFS made by *Leyden*, *Goude*, and *Rotterdam*, the diſtance EF between *Leyden* and *Rotterdam* to be 4883. In calculating this triangle upon his obſervations, I found the diſtance from *Leyden* to *Rotterdam* 6972, the ſame as he had marked in the preceding problem miſplaced, and the diſtance between *Goude* and *Rotterdam* 4883, which ſhewed me, that inſtead of *Leyden*, it muſt be read *Goude*, and that it is only an error of the preſs. But I know not how to reconcile theſe two determinations of the diſtance EF from *Leyden* to *Rotterdam*, which reſult from two different triangles, one of 6386, the other 6972. The firſt determination is the moſt immediate; but the ſecond is that, upon which he has eſtabliſhed his meaſure, and is verified by other triangles; but there is no angle obſerved at *Rotterdam*. We may therefore conclude, either that the obſervations of the angle of the firſt triangle AEF are faulty, or that he has miſtaken another place for *Rotterdam*; and then there would be no error in his meaſure.

This would be at leaſt a fact that would deſerve to be proved, and this might be eaſily executed by a perſon, who ſhould be upon the ſpot, by making a ſtation upon the top of the tower of *Rotterdam*, and obſerving from thence the angles between the *Hague* and *Leyden*, *Goude*, *Dort* and *Willemſtadt*.

Let us now examine the reſult of the diſtance from *Leyden* to *Rotterdam* determined by the firſt triangles. I have already ſaid, that the tower of *Rotterdam* declines from the meridian of *Leyden* $1^{c}\ 13'$ toward the eaſt, and ſuppoſing the diſtance EF, from *Leyden* to *Rotterdam* 6386 perches, as we have juſt found by the triangle AEF, we ſhall have the arch of the meridian between

Leyden and *Rotterdam* 6384, which being added to the arch of the meridian between *Leyden* and *Alkmaer* of 14215, gives the diſtance between the parallel of *Alkmaer* and that of *Rotterdam* of 20,599 *Rhinland* perches, or 39,767 *Paris* toiſes; and retrenching 60 toiſes, for the difference between the places where I obſerved and thoſe of *Snellius*, we ſhall have 39,707 toiſes for 42′ 5″, and for a degree 56,612 toiſes.

This meaſure differs very much from that which we found at firſt; it exceeds that of *Snellius* by 1600 toiſes, and is leſs by above 400 toiſes than that which M. *Picard* has determined between the parallels of *Sourdon* and *Malvoiſine*.

But, to leave nothing that may ſerve to clear this ſubject, I ſhall here examine what reſults from the obſervations made at *Alkmaer* and the *Hague*. I have not obſerved in the laſt town the height of the polar ſtar; but I have ſeveral times taken the meridian height of the ſun, by which I have determined the height of the pole to be 52° 4′ 13″. The height of the pole at *Alkmaer* drawn from the obſervations of the polar ſtar, is 52° 38′ 34″, the difference between the parallels of this town and of the *Hague*, is therefore 34′ 21″. *Snellius* has obſerved from *Leyden*, that the tower of the *Hague* declined 52° 22 from the meridian. The diſtance from *Leyden* to the *Hague* being therefore allowed to be 4103 perches, we ſhall have the difference between the parallels of theſe two towns 2505, which being added to 14,215, the diſtance from *Leyden* to *Alkmaer* upon the meridian, gives for the difference between *Alkmaer* and the *Hague* 16,720 perches, which agree with 34′ 21″ of latitude. Neglecting the difference between the ſtations of *Snellius*, and thoſe where I obſerved, becauſe they are hardly

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ſenſible; we ſhall have the magnitude of the degree 29205 *Rhinland* perches, or 56,382 toiſes; which gives a ſtill leſs determination than that which reſults from the laſt compariſon, wherein we ſuppoſed the diſtance from *Leyden* to *Rotterdam* 6386 perches, the ſame as we have found by the calculations from the rules which he relates.

V. *Remarks on the different manner of managing the common oars, and turning oars, lately propoſed by the Sieur* du Guet, *by M.* Chazelles *. *Tranſlated by Mr.* Chambers.

To judge of the effect of common oars, and the velocity they are capable of giving; we are to conſider them in a galley, a veſſel to which in all ages men have endeavoured to give all the advantages poſſible.

Now a common galley has 26 oars, on each ſide; each oar being 36 feet long: whereof 24 feet are out of the galley, and 12 within; but the part within is proportionably bigger, and beſides is fortified with wood in order to balance the part without: the *fulcrum* or point of ſupport, being on the edge of the galley. The end of the oar which enters the water, called the blade thereof, is half a foot broad, and about 5 feet long; ſo that each oar ſticks on a ſurface of water 2 feet $\frac{1}{2}$, and the 26 oars on 65 feet, and that there are 5 men to each oar. We may therefore conſider that 26 oars as all connected, acting at the ſame time, and impelling 65 ſquare feet of water with the ſtrength of 130 men.

The rowers exert unequal forces. He at the end of the oar works hard, as moving at each ſtroke

* March 1, 1702.

ſtroke or ſtretch of the oar, the ſpace of 6 feet, but the others leſs in proportion, and the next the *fulcrum* ſcarce ſtirs; ſo that when they are to continue rowing any time, they are obliged to relieve and ſucceed each other, which makes a little hindrance.

The ſtroke may be conſidered, as conſiſting of 3 parts: in the firſt, the men recover and raiſe themſelves; in the ſecond, they carry the oar forwards; the foreman advancing a ſtep, and ſtretching his body before the ſtern; in the third, they recline or throw themſelves backwards, with the arms upwards, in order to plunge the blade in the water: which laſt motion alone tends to make the galley advance. It muſt be obſerved, that at the ſame time the fall of the whole crew of 260 men, makes another impulſe or impreſſion on the galley, tending to ſink it deeper, which of conſequence muſt retard its velocity; ſo that the motion is performed in jirks, or ſhakes.

We have obſerved, that a galley rowed with all the ſtrength it can maintain for a continuance of time, in a ſtanding water, does not give above 24 ſtrokes in a minute: and that the firſt oar falls into the ſame part of the water, as the ſeventh: ſo that at each ſtroke the galley advances the ſpace of 6 banks, which is 3 * fathoms; and conſequently 72 fathoms in a minute, and 4320 fathoms in an hour, which makes 5 good miles or 1 league $\frac{2}{3}$ in an hour. This *calculus* I have compared with ſeveral experiments and obſervations, from all which I am confirmed, that a galley in a full calm, cannot go 2 leagues an hour, for a continuance: ſuch is the rate at which the common oars will carry us.

* It is *toiſes* in the original.

Now ſuppoſing the turning oars to be 12 feet from the centre of their motion, to the tip of the blade, making them dip 6 feet in water; and placing the point of ſupport 5 or 6 feet above the water line: the blade may be 3 feet broad, if required, or even more, which will impel 18 ſquare feet of water continually, and without interruption, with a force greater or leſs, according to the number of perſons applied to the handles, who all exert an equal force, and only move 3 feet, which they may continue to do much longer than the foreman of the common galley, who moving at double this rate, is ſoon thrown into a ſweat, and obliged to work naked.

The velocity of the galley in this caſe of turning oars, is eaſily eſtimated, if they only make one turn or revolution in 10 ſeconds, the velocity will equal that of the common galley, by reaſon the turn is equal to 12 fathoms: ſuppoſing, as we have done for the common oars, that the water does not give way in the leaſt. But for a greater juſtneſs in the eſtimate, we ſhould find by repeated experiments in known diſtances, how much the water yields or gives way in proportion to the velocity of the turns, and we ſhall have the greater accuracy the more of the turn of theſe oars exceeds the ſpace advanced at a ſtroke with the common ones.

There is no doubt but the ſtrength, for inſtance, of 100 men, acting continually on a bulk of water of 18 ſquare feet on a ſide, will ſoon put the largeſt veſſel in motion; ſince a common chaloop makes itſelf felt, notwithſtanding the inconveniences of ſuch towage.

I am perſuaded therefore, that ſuch oars would be of great advantage to large veſſels, and even more than to ſmall ones, by reaſon beſides the

great

great number of hands they have in readineſs to work them, there is more room for the ſlights of the handles, to make them move without any embarraſs, which cannot eaſily be done in a ſmall veſſel, where the ſpace between the two decks is but low, and uſually much encumbered.

VI. *An obſervation on a column of light ſeen at the obſervatory*, May 11, 1702, *in the morning*, *by M.* de la Hire *.

I obſerved a great luminous ray perpendicular to the horizon, and equal to the diameter of the ſun, in its whole height, which was about 9 or 10 degrees. This light appeared ſome time before the riſing of the ſun, and we ſaw it again after it roſe. The heavens were overcaſt with little clouds, lying along the horizon, which did not hinder the ſun from being ſeen very plainly; they only made little black bands and jaggs toward the edges: but the vertical diameter of the ſun at its riſing, appeared to me at leaſt equal to the horizontal.

In the memoirs of the academy for the year 1692, there is related an obſervation of M. *Caſſini*'s, of a light almoſt like this. M. *Caſſini* ſays, that this *phænomenon* is very rare, and that he never ſaw but one like it, which was in 1672. But both theſe obſervations having been made after ſun-ſet, he could not ſee the proportion of the ſun with this light.

We † may explain the light that I have obſerved in this manner. It is certain, that all the *parhelia*, and theſe appearances of light, never appear when the air is very ſerene, and that we ſee them generally toward the horizon, when it

* May 17, 1702. † Plate XVII. Fig. 1.

is full of little, long, thready clouds. Beſides, it is manifeſt, that there happens to the rays of the ſun, which met theſe clouds, the ſame thing, as that which we perceive when we look at the light of a candle through a glaſs that is a little greaſy; and when we rub it a certain way with the hand; for it then makes an infinite number of little furrows, of which the elevated part returns the light toward the eye, and we ſee theſe rays extended according to the perpendicular in the direction of the furrows. The ray of light muſt appear pretty near equal to the diameter of the luminous body; for only thoſe which meet the direction of the furrows perpendicularly, can be reflected toward the eye, the others which are oblique turning away from it, as we may try upon a thread of glaſs, by looking at a candle through it.

The ſame thing muſt happen to the little threads of the clouds, or to the little long parts lying this way, of which they are compoſed, as to the little furrows juſt mentioned.

VII. *Obſervations made by means of the burning-glaſs, by M.* Homberg.

The great burning mirrours, which we have uſed till now, have been concave, which reunite exactly the rays of the ſun, and make a very burning *focus*: but as this *focus* is made from reflected rays, which are united upwards, we are obliged to hold the ſubſtance in the air which we would expoſe to it, without being able to ſupport it in any veſſel. As ſoon as this ſubſtance is affected by the heat of the *focus* it begins to melt; when it is melted, not being ſupported by any thing,

thing, it runs and quits the *focus*, and conſequently receives no more impreſſion from it, ſo that we cannot make any continued experiment by theſe ſort of burning mirrours. Thus they have only ſerved for a curioſity without any uſe; this has made us wiſh for great glaſs *lens's*, thro' which the rays of the ſun paſſing, would make a *focus* downwards, to which we might expoſe the ſubſtance ſupported in convenient veſſels, as long as we pleaſe; which would not only give an opportunity of making continued obſervations, but alſo experiments, which are abſolutely impoſſible with the concave mirrours.

The duke of *Orleans* having ſix months ago brought one of theſe glaſs *lens's*, of 3 feet diameter, made by M. *Tſchirnhauſen*, one of our aſſociates, ordered me to make uſe of it for examining all ſort of ſubſtances, which I do as often as the ſun permits me. I relate here ſome of the moſt extraordinary obſervations that this glaſs has furniſhed us with; by which we ſhall ſee that gold and ſilver are volatile metals in the fire of the ſun, as other metals are in the fire of our furnaces.

Gold eaſily melts with the burning-glaſs; and it diſappears at length three ways, which differ from one another according to the degree of heat to which they are expoſed.

Fine gold reduced to a *calx* by ſpirit of ſalt melted with the ſun, fumes at firſt very much, and changes a part of it quickly into a glaſs of a very deep violet colour.

Fine gold, reduced to a *calx* by quickſilver, melted with the ſun fumes at firſt very much, and quickly changes a part of it into a tranſparent cryſtalline glaſs without colour; but if we hold this glaſs for ſome time in fuſion with the gold, it loſes its tranſparence, and becomes

opake by degrees, at firſt the colour of opal, then white as milk, afterwards it grows brown at the ſummit of the drop, and at laſt the drop of glaſs becomes of a deep brown, inclining to a green.

This glaſs ſwims upon melted gold, ſometimes whirling about all ways, ſometimes running over it in a right line, and waving, changing place with a very great velocity, without fixing to the veſſel which contains the gold, unleſs the veſſel itſelf begins to vitrify. Then the glaſs of the gold, and that of the veſſel mix together, and fix themſelves to the veſſel.

When the fine gold, that we would melt with the ſun, is not in a *calx*, but in a maſs, there does not at firſt appear any glaſs upon it, but it is formed by degrees; as thus:

The gold which I ſuppoſe to be pure, when firſt it is melted, appears in a drop, fair and clean like a looking-glaſs, but ſoon after its ſurface becomes as if we had thrown duſt upon it: this duſt very quickly gathers into a little drop of whitiſh glaſs, upon the middle of the melted gold, leaving the whole ſurface of the gold for a moment very clean and clear, as it was in the beginning of its fuſion; after which the ſurface of the gold appears again duſty: this powder at firſt covers the whole ſurface of the gold, like a general ſpot, which diminiſhes its breadth by degrees, but pretty quickly, till it terminates upon the middle of the maſs of gold, and enlarges a little the firſt drop of glaſs that was formed by the firſt duſt. It does this ſucceſſively, during the whole time that we hold the gold in fuſion to the ſun.

When the little drop of glaſs is come to the ſize of a very ſmall pea, its weight makes it run towards the edges of the melted gold, and then

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the dusty spots make a new little drop of glass, which becoming a little bigger, runs also towards the edges of the melted gold; this joins to the first and makes it bigger, and then the third little drop of glass begins to form itself.

The whole mass of gold will change this way into glass; but to the end that this may happen, it must be observed not to hold the melted gold exactly to the *focus* of the two burning-glasses; it is well to present it to it from time to time to strengthen the fusion, and then to take it away a little; for the true *focus* of our two glasses is too violent to hold long in fusion any metal whatsoever.

For the metals which are hard to melt, there are three ways of placing them to the *focus*, which produce three different effects. The first is to the exact point of the *focus*. The gold being held in this place a little while, begins to crackle and throw out little drops of its substance, to 6, 7, and 8 inches distance, the surface of the melted gold becoming very sensibly prickly, like the green shell of a chesnut.

All the substance of the gold loses itself this way, without suffering any alteration; for if you spread a sheet of paper under the vessel which contains this gold that crackles in fusion, we shall gather upon this paper a gold powder, of which the little grains being viewed through a microscope, appear to be little round balls of gold, which may be again melted into a mass.

The second way to place the gold in fusion, is to draw it a little from the true *focus*, till you see that the gold does not appear any longer prickly nor sparkles. In this place the gold vitrifies, as we have just mentioned, which is a true changing of the substance of a heavy, malleable,

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and ductile metal, into a light, brittle, and obscurely transparent glass.

The third way to place the gold in fusion, is to draw it still a little farther from the true *focus*, than it is in the vitrifying place, and here it only fumes; its loss there is very slow, and we are obliged now and then to bring it nearer to the *focus*, to prevent it from fixing.

These are the 3 different changes that fine gold suffers at the burning-glass; namely, to evaporate in fume, to change into glass, and to fly into the air by little grains.

It is pretty near the same with fine silver, but with these differences, that silver fumes much more than gold, that it goes away incomparably faster in fume, that it sparkles at the least heat, and that it does not vitrify entirely in the same manner as the gold.

Silver refined with lead fumes considerably, and the surface becomes dusty, as we have observed in that of the gold; but the powder which it makes does not melt into glass, as it does with the gold, for it is white and light as flower, it gathers in so great a quantity, that there is the thickness of more than $\frac{1}{2}$ a line over all the surface of the silver, when we hold it about a quarter of an hour together to the sun, and during this time a dram of silver has lost 26 grains, that is, more than $\frac{1}{3}$ of its weight.

Silver refined with antimony fumes yet more than that which is refined with lead, and the powder that it makes upon its surface, melts into glass as does that of gold; but this glass does not keep in a drop upon this silver, as the glass of gold does; on the contrary, it spreads over the whole surface, as if it was a yellow varnish. This glass is volatile, and evaporates in fume, with

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the maſs of its ſilver, in which it differs from the glaſs of gold, which does not go off in fume, and differs alſo from the powder gathered upon the ſilver refined with lead; for this powder increaſes more and more upon the ſilver expoſed to the ſun, but this varniſh does not ſeem to increaſe by expoſing it a great while to the ſun upon the ſilver.

When fine gold and ſilver have been for ſome time melted with the ſun, they are with difficulty melted with the common fire; and their diſſolvents do not diſſolve them ſo faſt, nor with ſo much ebullition as they did before, which is obſerved more ſenſibly in gold than in ſilver.

It would be well here to give the reaſon why a glaſs is formed upon gold and ſilver refined by antimony, and why upon ſilver, refined with lead, it only makes a powder which does not vitrify at all? Why theſe glaſſes and this powder have not the ſame weight as the metals, which have produced them? Why gold melted for ſome time in the ſun, melts with difficulty with the common fire? And why ſpirit of ſalt diſſolves it almoſt without ebullition?

To give the reaſon of all theſe facts, I find myſelf obliged to ſay before hand, 1. What the fire of our furnaces appears to be. 2. In what manner it acts. And, 3. the difference I believe there is between the common fire, and the fire of the ſun.

I ſay therefore, that the fire, which we commonly uſe, or flame, is nothing but a liquid compoſed of the matter of light and of the oil of the wood or coal; this fluid, or flame, is much lighter than the air that ſurrounds it, and being preſſed on all ſides but unequally by the air, it is driven by it continually to the ſide where it is

leaſt preſſed, which is uſually upwards with regard to us, or from the earth.

The particles of the flame are very ſmall, and capable of paſſing into the interſtices of the moſt ſolid bodies, being driven violently againſt theſe bodies by the air, of which the preſſure is more or leſs violent, according as this air is more or leſs condenſed by the cold, by the wind, or by an artificial blowing, ſuch as bellows, reeds, *&c.*

The violent paſſage of the flame acroſs the bodies, which are penetrated by it, diſorders and diſunites the parts of theſe bodies: this diſunion produces in ſome an entire diſſolution of their parts, as it happens to all bodies reduced to aſhes; in the others, it only produces a ſimple fuſion, as it happens in metals and bodies, that vitrify, of which the particles reunite and become again a ſolid body, as ſoon as the violence of the flame begins to ceaſe: but as the interſtices of theſe fuſible bodies preſerve the traces of the flame which has penetrated them, theſe interſtices remain greater or leſſer in the coagulation of theſe bodies, according as the flame has been more or leſs great, and as there remain more or leſs parts in its interſtices. Thus much for the common fire.

The fire of the ſun is only the ſimple matter of the light, which is diſperſed in the air, without the mixture of any oily ſubſtance of wood, or the like, driven by the ſun.

This matter being reunited by a burning-glaſs, and driven in a pretty large quantity againſt any ſubſtance whatſoever, penetrates it, croſſes it, and diſunites the parts almoſt in the ſame manner, as we ſee the common fire to act.

The firſt ſenſible difference of theſe two fires, conſiſts in this, that one, which is the ſun, is a ſimple matter, whoſe parts are infinitely ſmaller

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than thoſe of the common fire, which conſiſts, as we juſt ſaid, in a groſs mixture of the oil of the wood with the matter of the light.

The ſecond ſenſible difference of theſe two fires is, that the air, which is heavier than the flame, drives the flame according to the laws of the *equilibrum* of fluids, without which the flame would have no motion; whereas the fire of the ſun is driven by the ſun, without the air contributing any way to its action, which is plainly proved, becauſe the flame cannot ſubſiſt nor act in a place void of air, and becauſe the rays of the ſun act with as much violence in a *vacuum*, as in the open air.

Knowing therefore the principal differences of the nature of theſe two fires, we muſt examine alſo their different effects.

We have obſerved before, that the pores or interſtices of fuſible bodies, preſerve after their fuſion, the traces as well of the common fire, as of that of the ſun, which is plainly ſeen by the induration and boiling of the metals.

We have alſo obſerved that the flame, according as it is more or leſs thick, leaves in the pores of the body which it penetrates, a part of its ſubſtance, which is proved alſo, as well by the weight that certain bodies acquire in their calcination, as becauſe certain metals which are ſoft under the hammer, become brittle if we melt them, or if we make them red hot in a coal fire.

This being ſuppoſed, we muſt imagine that a metal, for example, gold, having been melted with the ſun, muſt have its pores or interſtices more cloſe, than if it had been melted by the common fire, ſince the ſubſtances, which have paſſed through the pores of theſe two different maſſes of gold,

are very different in thickneſs. And as theſe pores do not remain void, the matter which is introduced into thoſe of gold melted by the common fire, which are large, muſt be in a greater abundance than they are in the pores of gold melted by the ſun, which are ſmall.

We muſt alſo conſider, that the points of the ſpirit of ſalt, which are diſſolvents of gold, muſt drive out its foreign matter which poſſeſſes them, and that a greater quantity of this matter muſt go out of the pores which contain much of it, than there muſt out of thoſe which contain but a little.

And as it is only this foreign matter, which is introduced into the pores of a metal, that makes the bubbles which appear in the diſſolution of a metal, there muſt be much more ebullition in the diſſolution of gold that has been melted by the common fire, than in that which has been melted by the fire of the ſun. Beſides, we ſee that in the diſſolution of this firſt, there are many more of theſe bubbles very ſenſible, and that in the other, there are ſo few, that thoſe who have been preſent at this experiment have hardly ſeen any.

We have alſo obſerved, that gold which has been melted with the ſun, melts with much more difficulty at the common fire, than it did before it had been melted with the ſun. It is eaſy to underſtand the reaſon of it, if we ſuppoſe, as we have done, that the pores of gold melted by the ſun are more cloſe than thoſe of gold melted by the common fire, and that the parts of the flame, or of the common fire, are groſſer than thoſe of the fire of the ſun.

It muſt follow from hence, that the cloſe pores of gold melted by the ſun, give a more difficult paſſage to the groſs parts of the flame, than do the larger pores of gold melted by the common fire;

fire; or which is the ſame thing, the common fire with greater difficulty puts in fuſion the gold which has been melted by the ſun, than that which has not.

We have alſo obſerved, that the glaſs of gold is lighter than a like bulk of gold is. To underſtand the reaſon of it, we muſt imagine that the parts which a perfect metal is compoſed of, are of quickſilver, of a metallic ſulphur, and ſome terreſtrial matter, that the quickſilver is always volatile, and that the metallic ſulphur and the terreſtrial matter are fixed.

We may alſo imagine, that the parts of the matter of light, or of the rays of the ſun are ſmall enough to be introduced even into the compoſition of the metal, to diſunite the principles, among which the quickſilver being naturally volatile, finding itſelf diſengaged from the metallic ſulphur which held it, is carried away in fume by the violence of theſe rays: but that the metallic ſulphur being thus fixed, and remaining with the earth of the metal, they melt together and appear afterwards in the form of glaſs; ſo that in this glaſs of gold, there is only found the earthy ſubſtance of the gold, melted or vitrified by its ſulphur, and as the heavy part of a metal is its quickſilver, which does not make any part of the glaſs of the gold, this glaſs muſt be lighter than the gold itſelf which contains all its quickſilver.

We have alſo obſerved, that the earth of ſilver does not vitrify as does that of gold, which proceeds probably from this, that ſilver has much leſs ſulphur than the gold; that the ſulphur muſt ſerve for a flux to its earth, and that there is not enough found in the ſilver to put the earth in fuſion, and to vitrify it.

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This is confirmed by ſilver that has been refined by antimony, the earth of which vitrifies as does that of gold, becauſe there remains in this ſilver a part of the ſulphur of the antimony, which ſerves for a flux to this earth: but the ſulphur of antimony not being fixed as that of gold is, the glaſs which is made with the earth of ſilver is raiſed in fume with its quickſilver.

We ſee by theſe obſervations, that the idea which we had formed in chymiſtry of the invincible fixedneſs of gold and ſilver does not ſubſiſt any longer.

VIII. *A deſcription of the labyrinth of* Candia: *with ſome obſervations on the growth and generation of ſtones, by M.* Tournefort. *Tranſlated by Mr.* Chambers.

The labyrinth of *Candia* is a ſubterraneous paſſage, in manner of a ſtreet, which, by a 1000 turns and meanders on every hand, without the leaſt regularity, traverſes the whole inſide of a hillock, ſituate at the foot of mount *Ida*, 3 miles from the ancient city of *Gortina*. The entrance of this labyrinth is by an aperture 7 or 8 paces wide, but ſo low, that a man of moderate ſize can hardly paſs without ſtooping; the bottom of the entrance is very uneven, but the top quite flat, and naturally terminated by ſeveral beds or *ſtrata* of ſtones, placed horizontally over each other. The firſt thing we meet withal, is a kind of ruſtic cavern, the deſcent whereof is very gentle: but upon advancing further, the place appears perfectly ſurprizing. Among the multitude of turnings, there is one alley, much leſs intricate than the reſt, which by a path about 1200 paces

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long, whose extremity divides into two, leads to a large beautiful hall at the farther end of the labyrinth: to find this alley, we are to turn on the left, about 30 paces from the entrance, if any other passage be taken in lieu of it, after travelling a good way, we find ourselves engaged in a multitude of nooks and blindholes, which we have much ado to recover ourselves from.

In half an hour's time we advanced 1160 paces in the principal alley, without turning either to the right or left; it is 7 or 8 feet high, and roofed with a horizontal *stratum* of rocks, tho' in some places one is obliged to stoop a little, and particularly in a place about the middle to walk on all fours, it is commonly broad enough for two persons to walk in front, and the floor very even, without any considerable rise or fall; the walls are cut perpendicularly, or formed of stones which had obstructed the passage, and have since been ranged very neatly, as in walls where no mortar is used; but there are some partings to encounter withal, that a person would be inevitably lost, without the necessary precautions. As we had a great desire to come back again, we, first, posted one of our guides at the entrance of the cavern, with order to fetch people from the neighbouring town to come to our deliverance, in case we did not return before night. Secondly, each of us carried a large flambeau in his hand. Thirdly, we fastened papers, marked in all the passages on the right, which appeared difficult to be hit again. Fourthly, one of our *Greeks* left little bundles of thorns provided for the purpose on the left, and another took care to strew chaff along the road, of which he carried a bag full under his arm.

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By this means we made a happy journey, but after having well examined the place, we were all agreed, that there was no appearance of its being an ancient quarry from whence ſtones have been dug to build the cities of *Gortina* and *Gnoſſus*, as *Bellonius* and ſome modern authors have imagined, what probability is there, that they would ſeek for ſtones in the receſs of a narrow alley above a mile long, and interſecting by ſuch a multitude of other roads, which run through a whole mountain, that a perſon is in danger every moment of being loſt? They would much rather have opened the quarry in the uſual manner, as we find practiſed in the famous quarries of *Paros* and *Scio*; how would they get the ſtones along in a place only paſſable upon the hands and feet, and which yet is upwards of 100 paces long, and is certainly all natural? Add that the mountain is ſo rough and ſharp, that we had much ado to mount it on horſeback; we looked but in vain for the ruts of the waggons, which *Bellonius* aſſures us he had obſerved therein; ſuch ruts however could only ſhew, that they had made uſe of waggons to clear the alleys of the labyrinth, not that they had dug in it for ſtones for building; it may even be obſerved, that the ſtone of this labyrinth is neither hard nor beautiful, being of a dirty coloured white, like that in the mountain, at whoſe foot the city *Gortina* ſtands; and as to the city *Gnoſſus*, it was a great diſtance off.

'Tis much more probable therefore, that the labyrinth is only a natural paſſage, which ſome wealthy people had amuſed themſelves many ages ago with making paſſable, by opening moſt of the places which had been too narrow to raiſe the roof, they had only to pull down ſome beds of

ſtone, which naturally lie in horizontal *ſtrata*, through the whole thickneſs of the mountain; and they were at the pains of cutting the walls perpendicularly in ſome places, and to range moſt of the ſtones which obſtructed the paſſage of the low place, where we are to creep on all fours, they might poſſibly leave untouched, as a document to poſterity, what all the reſt had naturally been; for beyond this place the alley is as beautiful and commodious as on this ſide. As the ancient *Greeks* were ſtruck with every thing which had the appearance of grandeur, eſpecially in matters of building, 'tis probable they undertook to perfect what they found nature had only ſketched out. Some ſhepherds perhaps firſt diſcovered theſe ſubterraneous paſſages, and gave room for the great men of thoſe times to enlarge them; and thus to make that wonderful labyrinth, which now only ſerves as a retreat to bats, though it may anciently have afforded ſhelter to ſeveral families during the civil wars, or under the reigns of tyrants; for the place is quite dry, and void both of drains and congelations. It may be added to this conjecture, that there are two or three other very deep natural paſſages in the hills, near the labyrinth, which might be formed into the like wonders, if it were found worth while. Caverns are very frequent through the whole iſle of *Candia*; and moſt of the rocks, eſpecially thoſe of mount *Ida*, are pierced with holes, into which one may thruſt the head, and through them we frequently diſcern deep perpendicular caves; nor can any reaſon be alledged why there may not be horizontal paſſages between them, eſpecially in places where the banks of ſtones are laid horizontally over each other. 'Tis probably, that they who dug the amphitheatre of *Douvai* in *France*,

were invited thereto by ſome cavern, whoſe aperture might be like that of our wells, and the beauty, or perhaps the oddneſs of the place, engaged them to enlarge it, and give it the form of an amphitheatre, which ſtill poſſeſſes the cavity of a large mountain, all the outſides whereof are covered with earth; nor is this leſs wonderful than the labyrinth of *Candia*. ——— Be this as it will, 'tis certain the labyrinth, which is now ſeen in that iſland, is not the famous labyrinth mentioned by the ancients. This latter was built by *Dedalus*, upon the model of the labyrinth of *Ægypt*, which was one of the moſt celebrated buildings in the world, being adorned at its entrance with a vaſt number of columns, and 100 times bigger every way than that of *Candia*, as we are aſſured by *Pliny*, who adds, that there was no remains of this latter extant in his time.

I cannot quit the labyrinth without mentioning a very remarkable obſervation, which I had long been ſeeking for to confirm a ſyſtem, which I have formerly advanced concerning the vegitation of ſtones. Thoſe of this labyrinth do ſenſibly grow, and augment without any room for ſuſpecting any foreign matter employed externally to them; they who engraved their names on the walls of this place, which are all of living rock, and are hewn perpendicularly, little imagined, that the ſtrokes of their chizels would gradually fill up, and in proceſs of time, they ſhould become raiſed or prominent 2 or 3 lines above the naked of the ſtone; ſo that the characters, which at firſt were indented, are now ſo many baſſo relievos, the matter thereof is whitiſh, tho' the ſtone they ariſe from be greyiſh; and I can no otherwiſe conceive them, than as a kind of callus formed by the natural juice of the ſtone, which

which had gradually oozed or extravaſated into the wound, cut in their ſubſtance much like the callus formed between the fibres of fractured bones, tho' as the ſubſtance of theſe prominences is all rough, and as it were grained, it might likewiſe be compared to young fleſh, which every body knows riſes much in this manner. Something of this kind is alſo obſervable in the bark of trees, whereon names have been graven by the point of a knife; the poet had reaſon to ſay, that the characters grew as the tree grew.

Creſcent illæ creſcetis amoris.

Before my departure I ſhewed the academy an eagle's ſtone, wherein there were callus's of the like kind. Upon breaking this ſtone to obſerve its inward ſtructure, I found it covered in certain places with ſeveral ancient callus's, which had cloſed the parts which had been broken at the ſame time when it was growing. Theſe callus's were only the nutritious juice of the ſame ſtone, which, after uniting the parts, had thruſt about half a line thick beyond the ſame, and were at length hardened into a kind of ſolder. The ſame thing had befallen one of thoſe ſtones brought from the *Indies*, wherein we frequently find chryſtals, and ſometimes little diamonds. This ſtone having been cloven by ſome accident into ſeveral pieces, they had been knitted together again by a natural callus.

Theſe three obſervations evidently ſhew, that there are ſtones which grow in the quarries, and which conſequently are nouriſhed, and that the ſame juice which nouriſhes them, ſerves to rejoin their parts, when they happen to be broken. After the like manner, as is obſerved of the bones of animals, or the branches of trees, where care

is taken to close them with a bandage, this being so, we can hardly make any doubt, but that there are stones which are organized, for their nutritious juice can only be drawn from the earth; and this juice must have been filtrated thro' their surface, which we may here consider as a kind of bark, and conveyed thence into all the other parts. 'Tis more than probable, that the juice, which filled up the cavities of the characters graven in the labyrinth of *Candia*, had been conveyed to the surface of this rock from the bottom of its roots; nor is there any more difficulty in conceiving this, than in conceiving how the sap passes from the roots of the largest oaks and pines to the tips of their highest branches. The heart of these trees, 'tis certain, is extremely hard; and that of ebony guaicum, and the *lignum ferri* much harder. Coral is as hard in the sea, as out of it; and what we call sea-mushrooms, whose structure is so much admired, and which have the consent of all mankind for their growth, are really stone, and so like the common stone, that they are used in *America* to make lime of. No body, I suppose, will deny but that shells likewise grow, by means of a nutritious juice; yet this juice, as well as that, whereby the other hard bodies above-mentioned are fed, is conveyed into the tubes of such bodies how narrow soever they be, as easily as into those of plants, which are much softer.

It cannot be denied therefore, but that some stones feed themselves like plants; and there is even room to suspect, that they are likewise propagated after the same manner; at least we have a multitude of stones, whose generation can hardly be conceived, without supposing that they arise from a kind of seed, if I may use the term,

that

that is, from a germ, wherein the organical parts of thoſe ſtones are contained in miniature, as thoſe of the largeſt plants are in the embryos of their ſeeds.

The ſtones called *cornu ammonis*, *lapis judaicus*, toadſtone, *aſtroites*, ſerpent's eyes, with the *Bolonian* and *Florentine* ſtones, the ſeveral ſpecies of *pyrites*, ſea-muſhrooms, chryſtals of the rock, and an infinity of other ſtones, ſuppoſe their ſeveral ſeeds as much as common muſhrooms, truffles, and ſeveral ſpecies of moſſes, whoſe ſeeds have not yet been diſcovered; how ſhould the *cornu ammonis* come to be conſtantly in the figure of a ſcroll or volute, unleſs we ſuppoſe a ſeed wherein the ſame ſtructure was contained in little? Who was it that moulded it ſo accurately? Where are the moulds? So far from this, that theſe ſtones are found in the ground like common pebbles, notwithſtanding all the enquiry I have been able to make in *Provence*, *Poictou*, and *Normandy*, where theſe ſtones are common enough, I have found none of their moulds, nor any thing like them. The ſtructure of the metallic *cornu ammonis* is ſtill more remarkable than that of the ſtony ones; the former are likewiſe ſpiral; but there are ſome ſpecies of them, where each ſpire conſiſts of ſeveral pieces, articulated together by ſuitures, like thoſe of the ſkull, as is eaſily perceived upon breaking them.

The *lapis judaicus* is in the figure of an olive, but flatted on the outſide, and roughened with little grains; in breaking it always cleaves obliquely, and glitters like talc to the *genus* whereof it muſt be referred; ſince by calcination, it becomes plaiſtered, like the ſpecies of talc, called *lapis ſelenites*: now the jews ſtones, are not certainly moulded thus, and conſequently recourſe muſt be had to their ſeed.

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The toad-ſtone, and thoſe called ſerpent's eyes, which are naturally of a beautiful poliſh are alſo formed by their peculiar ſeeds, faſtened upon rocks, which furniſh a juice proper to dilate them. The ſeveral ſpecies of eagle-ſtones, which are commonly in figure of an egg, and have a chore in their cavity, like the ball of a little bell, cannot be produced without a ſeed; and the ſame may be ſaid of the *belemites*, otherwiſe called *lapis lynſis*, wherein we find *radii* iſſuing from the ſame centre, and terminating in the circumference and a baſe, uſually hollowed in form of a cone. Such a ſtructure ſuppoſes either ſeeds, or moulds; but no moulds are found; or if they were, who was there to break them, in order to diſengage the ſtones? If we ſometimes find theſe kinds of ſtones in rocks, 'tis by reaſon the rock happened to involve them in its growth; as likewiſe befalls thoſe, called ſhell-rocks, whoſe geneſis may be explained from the inſtance of thoſe ſtones, which are ſometimes found in the trunks of large trees.

The aſtroites is called by *Geſner lapis aſtereas*, by reaſon its figure always conſiſts of ſix *radii*. Thoſe ſtarry ſtones, which are radiated like a fiſh's bones, are commonly found ſeveral of them faſtened together, in horizontal *ſtrata*. The ſtones called *entrochi*, are alſo in *ſtrata*, but their circumference is round. Some of them are jointed together, as it were, with mortices and tenons. The ſpecies of *pyrites*, both oval, ſpherical and cylindrical, whoſe ſurfaces are either poliſhed, or cut like diamonds, are penetrated by *radii*, which terminate in a ſpecies of *axis*, which paſs thro' their centre, traverſes them from pole to pole. Theſe *pyrites*, I ſay, were not certainly caſt in a mould, any more than the ſtones of *Bologne* or *Florence*,

Florence, which commonly repreſent the ſame landſcapes, or the ſame ruins of towns; and it is not more probable, that thoſe ſpecies of agates, which, from their repreſenting little ſhrubs, or foliages, *dendroides*, ariſe from their peculiar ſeeds. Theſe ſtones are found in the earth ſeparate from each other.

Chryſtals of the rock may likewiſe probably be produced from ſeeds. Theſe chryſtals are naturally cut in flat ſides, and their figure is conſequently the ſame in all the ſame ſpecies; that is, all the pieces of the ſame block of chryſtal conſiſt of 3, 4, 5, 6, or 7 faces; yet there is no room to ſuſpect either their being moulded, or formed by any coagulation like chymical ſalts: for, beſides that we evidently ſee theſe chryſtals come out of the rock, where they are faſtened variouſly againſt the ſides of caverns, with their points turned upwards, downwards, or ſide-ways. It cannot be alledged, that the juice, which produced theſe ſtones, had been caſt in the caverns like diſſolutions of nitre, for inſtance, evaporated in earthern pans, that of the chryſtals muſt have neceſſarily paſſed thro' the rock; and we cannot ſuppoſe that it paſſed all at once, and fixed thereon by little and little, eſpecially if we conſider, that there are pieces of chryſtal, which weigh upwards of 60 pounds, as is obſerved by *Hottinger*, in the country *Valais*; thoſe brought from *Madagaſcar* are very maſſive. Father *Kircher* aſſures us, he had met with ſome which weighed above 100 pounds; and *Pliny* relates, that *Livia*, the wife of *Auguſtus*, carried ſome to the capitol, which weighed above 50 pounds. If ſo large a quantity of liquor was ſhed all at once, out of the pores of rocks, 'tis evident, it would run down every way, and form a lump of ice, in

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lieu of a cylindrical body, regularly hewn in flat ſides. 'Tis certain therefore, that the juice, which contributes to the growth of the chryſtal, tranſpires by little and little through the rock; and this being ſo, how can we conceive it to riſe in tall columns, from an inch to upwards of a foot high, without ſuppoſing ſeeds, which gradually ſwelling by the nutritious juice they receive from the rock, unfold and diſplay the regular ſtructure which they included, perhaps, under the ſurface of a point. There ſeems to be a good deal of relation between the *geneſis* of theſe chryſtal columns, and that of teeth; each ſeed, as it ſwells, may perhaps form a kind of hexagonal trunk, whoſe inſide only hardens by little and little. This ſtructure might be treated as imaginary, were it not known that diamonds themſelves cut more eaſily one way than another; that marbles have their veins; and that rock-chryſtal has its pores open enough, to imbibe the colours which are given it; but after ſeveral enquiries upon the figure affected by chryſtals, concludes it as natural to thoſe ſtones, as that of the leaves and flowers of plants; and he attributes both the one and the other to an architectonical ſpirit, and a plaſtic power, how much eaſier is it to ſuppoſe a kind of eggs, ſince every body agrees, that the ſeeds of plants are as much eggs, as the parts of birds or fiſhes, which, in all ages, have bore that name? And what is an egg, if it be not the bird, fiſh, plant, or perhaps ſtone in miniature? We may therefore ſuppoſe, that chryſtals vegitate, like ſeveral other ſtones; that is, begin with a ſeed, or germen, and that the ſame juice, which is communicated to them by the rock, from whence they ariſe, hatches and makes them grow

as far as their ſolid texture is capable of ſtretching; and what can we think of thoſe found near *Allenſon* and *Medoc*, the former whereof are hexagonal and pyramidal, at the two ends having a colour withal much like diamonds, and found in ſprings; while the latter are almoſt oval, of a darker colour, and found in the ground; do not both of them ſuppoſe real ſeeds, as much as thoſe naturally found in the lentil form, or which, with their lenticular figure, have a ridge like an aſs's back?

We are not to conclude that rock chryſtals are form'd as large as we find them, from there being piles of hay, hogs briſtles, and the like matters ſometimes found in them, for beſides that what we call hogs briſtles, *&c.* may only be defects of matter found in the ſeeds, 'tis poſſible that theſe ſeeds in the courſe of their growth, may have fallen upon ſuch bodies, and gradually incloſed them.

Iſicles begin by a round hollow tubercle, which in thoſe ſuſpended from above downwards, lengthens as a tube, and grows or ſwells by rinds, like the ſtems of young trees; ſuch as begin from below upwards, grow likewiſe by rinds, but their cavity neceſſarily fills up, on account of the ſituation; the congelations themſelves begin with a kind of germ or ſemen, and 'tis probable the generality of theſe germs remain hollow ever after.

What we call *fluores lapidum* may perhaps be referr'd to the ſame cauſe, eſpecially ſuch as are form'd in thoſe oval or roundiſh pebbles found in the *Levant*, ſeparate from each other, their outer ſurface is ſmooth, and as hard as flint, but the inſide is hollow and lin'd with chryſtals or other matters, whoſe figure and colours are wonderfully beautiful; does it not appear that

their germs had been dilated by little and little, and their parts open'd and diſplay'd from each other, by means of a juice imbib'd from the earth.

That immenſe quantity of common pebbles which covers the *Craudarles*, ſeems to confeſs the ſame principal; that tract of ground, which is upwards of ſeven leagues around, is ſo full of roundiſh pebbles, that we find them in plenty whereſoever we dig. M. *de Peireſc*, who firſt ſtarted this notion of the ſeeds of ſtones, tho' he ſhewed that term in a different ſenſe from ours, alledges this plain as a proof of his opinion, for how ſhall we ſuppoſe all theſe pebbles were form'd; it muſt not be pretended that they are as old as the world, unleſs we maintain, that all the ſtones upon the earth were produced at the ſame time, whereas the obſervations already deliver'd upon the vegetation of ſtones, afford a proof that there are new ones produced every day; and the ſame M. *de Peireſc*, while he was yet very young, made a notable remark to the ſame purpoſe; bathing one day in the *Rhone*, near the city *Avignon*, he perceived that the bottom of the river was become all uneven, and cover'd with little ſoftiſh pebbles like hard eggs, ſtripped of their ſhells; but he was ſtill farther ſurpriz'd, when he found ſome days after, that not only thoſe which he carried home with him, but thoſe which had remained in the *Rhone*, were become as hard and ſolid as any other pebbles on the edge of the river; he ſuppoſed that theſe germs or young ſtones had been turn'd up by an earthquake, which was felt ſome days before, and had thrown them out of the bowels of the earth.

To

To theſe obſervations may be added another which we made in an iſland of the *Archipelago*, called *Antiparos*, becauſe ſituate overagainſt the famous iſland *Paros*. From the bottom of one of the fineſt grottos imaginable, lined all over with beautiful congelations, ariſe ſeveral marble pillars, the higheſt whereof is upwards of ſix feet, and one foot in diameter, being pretty cylindrical, and equally thick every where; there are other little ones all about it like ſo many young horns, and not far off there is half a one remaining, which had been broke off, and now repreſents the trunk of a felled tree; the middle, which is a ſpan broad, conſiſts of a browniſh marble, and repreſents the woody part of the tree; this is ſurrounded with a blea and bark, and even with old bleas of different colours, which are diſtinguiſhed by ſix concentrical circles, two or three lines thick, whoſe fibres proceed from the centre to the circumference; theſe marble trunks ſeem to have vegitated, and perhaps ſtill continue to vegitate; for beſides that there fall no drops of water in this place, 'tis inconceivable that any ſuch drops falling 25 or 30 fathoms high, ſhould have been able to form regular cylinders, terminated uniformly domewiſe. In the ſame grotto to the left hand we find a pyramid more ſurpriſing than any thing, it is 24 feet high inſulated, and made like a tiara, being adorn'd with ſeveral capitals in relievo, which are fluted lengthwiſe, and ſuſtain'd on their pedeſtals. This pyramid, whoſe baſe is 12 or 15 feet broad, is all full of enrichments, the tops whereof are bigger than the bottoms; and it may be obſerved that their branches, like thoſe of cabbage flowers, ſhoot from below upwards, and terminate in

large cluſters; 'tis next to impoſſible all this ſhould be perform'd by the drop of falls of water, where the laſt would always cover the work of the firſt.

What has been ſaid concerning the generation of ſtones, may be extended to metals; 'tis very probable theſe bodies are likewiſe propagated by their ſeveral germs; nor will this conjecture be thought too daring by ſuch as will obſerve this natural vegetation of pure gold, which has ſhot, in manner of foliages, thro' a hard, as it were, chryſtallized ſtone; there likewiſe preſents you a piece of ſilver, which ſpringing of it ſelf thro' a piece of chryſtal, ſubdivides into ſeveral threads, all of which catch upon pieces of the ſame chryſtallization. Another piece which I offer you, ſeems ſtill more ſurpriſing, being little germs of ſilver inclos'd in a lump of marble; theſe germs are ſhap'd like flat plates, about the third of a line thick, radiated like fiſhes bones. A little piece of copper which I likewiſe lay before you, has ſhot into branches in the ground, as here you find it; 'tis ſcarce poſſible to explain all theſe productions, by veins of metals running in the viſcera of the earth; nor can it be objected that theſe foliages have no determinate figure, and are only imperfect vegetations; let their name be what it will, our buſineſs here is to account for their generation, ſuppoſing there were fluid metals in the ground, they could not paſs thro' pores of chryſtallized rocks, and thus ſhoot into foliages.

The *arbor Dianæ*, or thoſe icy branches found on the glaſs in a ſudden fit of froſt ſucceeding a fog, will give no aſſiſtance towards explaining theſe *phænomena*; every body knows that 'tis with fogs much as with diſtill'd waters: if a

piece of linnen dipt in cold water be applied on the capital of a glaſs alembic, the ſpirituous parts of the diſtilled matters having more motion than the reſt, will be reflected back again, and paſs thro' them in different ways, ſo as to form tolerable ramifications; but what imports all this to rock chryſtals, for inſtance, which line the ſides of a cavern as well as the top, and are always ſhaped in one uniform manner; the inſtances above do indeed ſhew, that all things which are naturally figur'd, do not ariſe from any peculiar ſeeds, but this is what we have not alledged, and how ſhould the inſtance of branches on glaſs be applied to account for metallic vegetations, will any one ſay they are form'd of vapours riſing in caverns? vapours would make a metallic cruſt or ſtratum, inſtead of leaves of gold or ſilver, which have ſeveral inches projecture, and whoſe roots penetrate the rock. As to the *arbor Dianæ*, all naturaliſts are agreed that it ariſes either from chryſtaliſations of particles of nitre, to which metallic particles precipitated on account of the weakneſs of their diſſolvent faſten themſelves, or from the effects of mercury, which by the heat it is agitated with, carries off the particles of the metals it is amalgumated withal; our ſubject is of a quite different nature, the foliages I juſt preſented to you being perfectly ſolid, they are pure gold, ſpringing from a hard rock, where nothing like what paſſes in the *arbor Dianæ* can be ſuſpected.

Among the ſeeds of ſtones and metals, there are ſome which not only ſoften by the juice of the earth, but become quite liquid, and if in this ſtate they penetrate the pores of certain bodies, they harden and petrify therein, as has befallen

befallen the piece of fern here preſented, as well as theſe lobſters, which are turn'd to meer ſtones by this accident. If the ſtony ſeeds be lodged in the cavities of certain bodies, they harden and aſſume the figure or moulding thereof; thus what we call *pectinites*, *conchites*, *mytulites*, *oſtracites*, *nautilites*, *echinites*, are real ſtones, whoſe liquid ſeeds had been received into the cavities of the ſhells, called *pecten*, *conca*, *mytulus*, *oſtrea*, *nautilus*, *echinus*, &c. whoſe figure they ſtill bear in theſe *cochlites*; we ſee a piece of a ſnail, and there are multitudes around *Paris*, where the ſhell ſeems as if it had been inſenſibly turn'd to duſt; when the ſeeds of different ſtones happen to be mingled together, they always retain their proper characters; thus the ſeed of chryſtal produces chryſtal, and what was intended to form ſtone produces ſtone, as we may ſee in this *cornu ammonis*, and this *conchites*, whoſe cavities are all chryſtallized.

On the contrary, if theſe liquid ſeeds be ſhed on pebble ſhells or ſand, they incloſe thoſe bodies by degrees, and inſinuating between them, form a kind of cement, which continues growing, notwithſtanding its hardneſs, like other living ſtones. 'Tis probable that theſe rocks, which are only cluſters of pebbles cemented together, were originally form'd by a multitude of ſuch liquid ſeeds like the quarries, which are full of ſhells, unleſs we ſuppoſe that the rocks had incloſed theſe bodies in the courſe of their growth.

We alſo find the ſeeds of real ſtones in the ſpawn of certain ſhell fiſhes, together with that hard ſolid matter intended to make the covers of thoſe animals. All the fiſhes incloſed in ſhells either ſpawn or lay eggs, but there are few of this latter kind, and I know of none but the buccinum,

cinum, which is reputed oviparous; be this as it will, the ſeeds both of the one and the other muſt contain the matter of the ſhell, how hard and thick ſoever it may afterwards become, as much as the ſeed of an elephant includes all the ſolid maſſive bones thereof. There is a kind of ſhell called pholas, never found but in the cavities of pebbles, which cavities are of the ſize proper to receive them; yet it is hardly conceivable that theſe fiſhes ſhould have come and dug their cells, we rather ſuppoſe that the ſtones they are incloſed in were once ſoft, and that the kind of jelly they began withal was found in the ſpawn, as the matter which afterwards becomes the egg-ſhell, is found in the firſt ſeed thereof; and certainly the ſhell of an oſtrich's egg is incomparably harder than the rocks we are ſpeaking of.

After all the obſervations hitherto rehears'd, we may ſuppoſe the ſeed of ſtones and metals to be a kind of duſt, which perhaps ſeparates from the ſtones and metals while they are yet alive, that is, while they grow, as we have found that ſome of them do really grow. This ſeminal duſt of ſtones may be compar'd to the ſeeds of ſeveral plants, for inſtance, thoſe of ferns, capillaries, moſſes, truffles, and the like, whoſe ſeeds were never yet diſcoverable by a microſcope; yet theſe ſeeds propagate as well as the largeſt, and pebbles may perhaps be among ſtones, what truffles are among plants. This notion is not quite new; *Pliny* aſſures us, that *Mutianus* and *Theophraſtus* believ'd that ſtones produced other ſtones; and *Gregory Nazianzen* aſſures, that there were ſome authors who held that ſtones made love.

How many fiſhes are there, whoſe ſpawns are at leaſt as ſmall as grains of ſand? yet the

the naturaliſts allow that whole fiſhes are couch'd in miniature, in the germs of theſe grains of ſand; the buſineſs here therefore is only as to more and leſs, and who doubts but that the author of nature, who has inclos'd the huge fiſh narwel, called the ſea unicorn, which is upwards of 20 feet long, in the germ of an egg a $\frac{1}{4}$ of a line big, may not have inclos'd a bank of ſtone, in a germ no bigger than a grain of ſand. Nothing ſhews the grandeur of the deity more, than this ſimplicity and uniformity found in the production of all bodies; what can be more glorious than to ſee men, fiſhes, birds, quadrupeds, reptils, plants, ſtones, and metals, all ſpring from inſenſible molecules? And as there are ſtones which evidently grow by an inward principal, depending intirely on their particular organization, which receives and diſtributes the juice, furniſh'd them by the common mother of all creatures, why may we not admit the ſame principal in the other foſſils? not that we are to conceive the largeſt mountains as only conſiſting of a ſingle piece; thoſe huge maſſes of rocks are uſually compoſed of a multitude of ſeparate blocks, which were produced by ſo many germs, or perhaps by ſeveral germs, which had blended together as they grew. Banks of ſtone are commonly horizontal, and thoſe which are vertical or oblique, 'tis probable are only render'd ſuch by ſome extraordinary ſhock; we may ſuppoſe that it is the preſſure of the atmoſphere, which bearing equally on the ſurface of the earth, makes the growing ſeeds always ſpread horizontally.

IX.

IX. *An eſtimate of the power neceſſary to move boats, both in ſtagnant and running waters; either by a rope, or by oars, or any other machine; by M.* de la Hire *. *Tranſlated by Mr.* Chambers.

The power neceſſary to tow a boat in ſtagnant water by a rope faſtened thereto, and pulled by a perſon on the ſhore, is the ſame as is required merely to hold or ſuſtain the boat in a running water, whoſe velocity is the ſame, as that wherewith the boat is towed in the ſtagnant one.

This propoſition is ſelf-evident; by reaſon the impulſe or reſiſtance of the water againſt the boat, is the ſame in both caſes.

But it is likewiſe the ſame, either to tow a boat in a dormant, or to hold it in a current water, tho' the power be placed in the boat itſelf, and act by a rope faſtened to ſome fixed point, ſuppoſing the velocity of the water impinging on the boat, to be equal in both caſes.

To determine the power neceſſary to this effect, we muſt know the ſurface of the boat, which is preſented to the direction of the motion of the water; and the velocity wherewith either the boat is pulled, or the water runs.

What is known of the nature of water and of motion, we here ſuppoſe as principles to build on: as,

1. That heavy bodies in falling any given height, acquire thereby a velocity, which uniformly continued, would carry them double the ſpace in the ſame time that their accelerated velocity has carried them.

* Nov. 22, 1702.

2. That theſe velocities are in a ſubduplicate *ratio*, that is, are to each other, as the ſquares of the ſpaces they paſſed thro' in falling.

3. That the ſame laws of motion obtain in fluid bodies, as in ſolid ones.

4. That a heavy body falling with an accelerated velocity, moves 14 feet in a ſecond of time.

Hence we learn, that water, after falling 14 foot high, will have acquired a velocity thereby, which would carry it 28 feet in a ſecond of time; and conſequently that water, iſſuing from the aperture of a tube 14 feet below the level of the water of a reſervoir, has a velocity which will carry it with an uniform motion, the ſpace of 28 foot in a ſecond of time.—Which property of water is confirmed by all experiments.

From an experiment made in the middle of the river *Seine*, and the moſt rapid part thereof, M. *Mariotte* found, that it ran 3 feet $\frac{1}{4}$ in a ſecond of time. From a ſecond experiment, made in the ſame place, to learn the force of the water in that velocity; by making it ſtrike or puſh againſt a thin plate 36 inches in ſurface, he found the ſame equivalent to a weight of 3 pound and $\frac{3}{4}$.

From theſe experiments, together with the principles above laid down, may ſeveral conſequences be reduced.——— For ſince a heavy body falling 14 foot in a ſecond of time, acquired a velocity thereby capable of carrying it in the ſame time, with a uniform motion, the ſpace of 28 feet: ſuppoſe * A D the height of a reſervoir 14 feet, the water muſt ſpring out in D, with a velocity, which uniformly continued, would carry it 28 feet in a ſecond of time. And ſuppoſe it were

* Plate XVII. Fig. 2.

enquired,

enquired, what height the reſervoir A B muſt have, that the water in B may ſuſtain 3 pounds $\frac{3}{4}$ under a ſurface of 36 inches, or $\frac{1}{4}$ of a foot? It will be found, that taking 17 pounds for the weight of a cubit foot of water, AB need only be the height of 31 lines, or 2 inches 7 lines nearly.

The ſame lines AD, and AB, being ſuppoſed together with the velocity in D, ſuch as above determined; I ſay,

1. *Rule for finding the velocity of water from the height of the reſervoir.*

That the root of AD, is to the root of AB, as the velocity in D to the velocity in B; or which is the ſame thing, AD is to AB, as the ſquare of the velocity in D to the ſquare in the velocity in B.

And in our inſtance, we ſhall have 14 feet, or 2016 lines, a conſtant number; or elſe 18 to 31 lines: as 784, which is the ſquare of 28 feet, that the water in D is to run in a ſecond, and which will be alſo a conſtant number, or even 7 to 12, and ſomewhat more, which will alſo be the ſquare of the ſpace, which the water is to move in a ſecond in its fall from A to B.

But the ſquare root of 12, is ſomewhat leſs than $3\frac{1}{2}$, which therefore is the way in feet that water moving uniformly will paſs in a ſecond of time, under the given depth of AB below the level of the reſervoir.

Hence we advance, that water running in water itſelf, has the ſame velocity, as if iſſuing at an aperture at a certain depth below the ſurface of a reſervoir, which is determinable by the ſtroke or impulſe of the water againſt an oppoſing body: which is a piece of knowledge of great conſequence, for aſcertaining the motion and impulſe

of waters, which run at large, and unconfined by any obſtacle. By means hereof we may learn the different efforts or impulſes of the water againſt floating muſcles, and of conſequence the ſtrength required to hold, *e. gr.* a boat in running water, or make it proceed either in ſtanding or running water; the velocity of the water againſt the boat in the ſeveral caſes being given, ſuch power being reducible to that required to ſuſtain a load of water at a certain depth below a reſervoir, determinable by the velocity propoſed.

But in examining the reſiſtance of the water againſt a boat; in lieu of a boat we ſuppoſe a plain ſurface perpendicular to the ſtream or current of water; which will amount to much the ſame as that irregular ſurface, which the boat preſents thereto.

Suppoſe then, a ſurface immerged vertically in a running water, the current whereof ſtrikes directly againſt the ſame, and that in the middle thereof, a cord is faſtened, whoſe other extreme is hung on a fixed point G *. Here it is evident, the force wherewith the water impels or ſtrikes againſt the ſurface B, is the ſame as that wherewith the fixed point G is pulled or drawn: by reaſon the whole reſiſtance of the ſurface B, to the current of water is derived originally from this fixed point, without which there would be no reſiſtance at all.

To find what this power is, the ſurface B, as alſo the velocity of the water, or the way it makes in a ſecond of time being given, we muſt uſe,

* Fig. 3.

2. *Rule for finding the force of water from its velocity.*

Take the ſquare of the number of feet the water moves in a ſecond of time, and divide it by 56, which is a conſtant number, and obtains in all caſes; the quotient will be the number of feet, which the water muſt be ſuppoſed above the given ſurface, to find the effort or impulſe thereof. For multiplying this height by the given ſurface, the product is the cubic or ſolid bulk, which will be ſuſtained thereby, which is the force required.

Now for every cubic foot we are to take 17 pounds; and for every cubic inch 5 drams $\frac{320}{1728}$, tho' in ordinary computations, it may ſuffice to take 5 drams $\frac{1}{3}$, which amounts to nearly 17 pounds the foot, which number we ſhall keep to in what follows.

If the velocity be given in inches, and the ſquare thereof be divided by 56, the quotient will be the number of lines which the water is to be above the propoſed ſurface.

To demonſtrate the rule, it has been already noted, that the velocity of water iſſuing at an aperture 14 feet below the ſurface of a reſervoir, is ſuch as carries it 28 feet in a ſecond of time, and conſequently the ſquare of the velocity of 28 feet, is to the ſquare of the number of feet, which will be moved in a ſecond, with the propoſed velocity; as the height of the reſervoir, 14 feet, is to the height required: or thus; the ſquare of the velocity of 28 feet is to the height of 14 feet, (which is a conſtant *ratio* in all caſes, and is the ſame as 56 to 1) as the ſquare of the feet moved with the propoſed velocity, to the required height of the water, in feet. But if in lieu of feet, you take the given velocity in inches, the

the number muſt then be divided by 56, multiplied by 12, to have the inches dividing only by 56, the quotient will be but twelfths of inches, *i.e.* lines.

Suppoſe, for an example, a velocity of water propoſed to be ſuch as carries it 6 feet in a ſecond of time. The ſquare of 6 is 36, which divided by 56, gives $\frac{3}{5}\frac{6}{6}$ or $\frac{9}{14}$ of a foot; amounting to 92 lines $\frac{1}{2}$, or 7 inches, 8 lines $\frac{1}{2}$, for the height of water above the propoſed ſurface. And the ſame ariſes upon ſuppoſing 72 inches velocity in lieu of 6 feet.

Now, if the propoſed ſurface be 36 inches ſquare, that number muſt be multiplied by the height found, which will give a ſolid bulk of 277 inches $\frac{1}{2}$, and a weight of 480 drams, or 11 pound, 9 ounces of water: the quantity of the effort or impulſe of the water againſt the ſurface propoſed.

It follows hence, that if the ſurface, which a boat preſents to the current of water, be one fathom or 36 feet, which is 144 times more than that of the foregoing inſtance, and the velocity of the water the ſame, *i. e.* moves 6 feet in a ſecond; there be a power required 144 times greater than the above power of 11 pound, 9 ounces, which will amount to 1665 pounds. Such power therefore of 1665 pounds, will be required to hold or retain a boat of the magnitude here ſuppoſed, in a water that runs at the rate of 6 feet in a ſecond.

If the water only make 4 feet in a ſecond, (which may be the velocity of the *Rhone*, there being no ordinary river that runs ſo faſt as 6 feet in a ſecond) and the boat be ſuppoſed to run againſt the ſtream at the rate of 2 feet in the ſame time; it follows, that the water will ſtrike againſt the boat with a velocity of 6 feet in a ſecond; ſo

that

that on this ſuppoſition a power of 1665 pounds will be required.

An eſtimate of the ſtrength of horſes employed in towing or drawing barges, boats, and other bodies, either in running or ſtanding waters.

A large barge on the river *Seine*, preſents about three fathoms, or 108 feet ſurface to the ſtream of water, which, taking it about the middle of the river, may be computed to move at the rate of 2 feet $\frac{1}{2}$ in a ſecond of time; 12 horſes yoaked to ſuch a veſſel tow it againſt the ſtream at the rate of 1 foot $\frac{1}{2}$ in a ſecond; which amounts to 900 fathom in an hour, and 9000 fathom, or 4 leagues in 10 hours; the ſpace of time the horſes are able to work *per* day.

Now on a ſuppoſition, that the ſtream of water runs 2 feet $\frac{1}{2}$ in a ſecond, and that the barge riſes againſt it at the rate of 1 foot $\frac{1}{2}$ in the ſame time, the velocity of the water impinging, or ſtriking on the barge, may be raiſed at 4 feet in a ſecond; agreeably now to the foregoing rule, the ſquare of 4 feet, or 48 inches, being 2304; this divided by 56, yields 41 $\frac{1}{7}$, the height of the water in lines amounting to 3 inches, 5 lines $\frac{1}{7}$. This height, therefore, multiplied by the ſurface of the barge 108 feet, yields 26 cubic feet $\frac{1}{3}$ of water very nearly. So that ſuppoſing the cubic foot of water to weigh 72 pounds, we ſhall have 1896 pounds for the quantity offered, required to tow the barge in the preſent caſe; conſequently each horſe muſt ſuſtain an effort of 158 pounds, on the ſuppoſition of its walking one foot $\frac{1}{2}$ in a ſecond.

What is here ſhewn of a running water will hold equally of a ſtanding water; ſuppoſing the barge to be towed therein at the rate of 4 feet in a ſecond,

cond, the velocity of the water ſtriking againſt the barge, being the ſame in both caſes.

And further it will be the ſame ſtill, if you ſuppoſe ſome fixed point, either in a running or ſtanding water; and that the barge being faſtened thereto by a rope, is pulled towards the ſame by a power placed in the barge itſelf. For the buſineſs here, is only to overcome the reſiſtance of the water ſtriking againſt the barge with a certain velocity, which determines the quantity of the power.

But of all the various ways of applying a power to a machine, in order to pull a body by a rope, 'tis certain, there is none whereby any thing can be gained. For if the power be ſmall, it will pull the body faſtened to the rope the more ſlowly; if great, the more ſwiftly, and ſuppoſing the ſame power to be employed, the motion will always be in proportion to the time.

We now proceed to the motion of boats, and other veſſels rowed with oars: but by the way, it may not be amiſs to note, that all ſtrength ariſes from ſome reſiſtance, as may be ſeen in levers: for if you pull one end of a lever kept faſt by the other end, the effort you make on the middle parts of the lever, is derived wholly from the reſiſtance of the part where the lever is fixed. It being evident, that if this end was not fixed, you could make no effort on the middle of the lever, by any power applied to the other end. In like manner, if you pull a rope, in order to move a weight faſtened thereto, you will not ſtir it in the leaſt, whatever power you employ, otherwiſe than by an effect ariſing from the fixed points where you ſtand; and you exert an equal power to puſh that fixed point towards the body, as to pull the body towards the fixed point.

Suppoſe now two boats in a ſtanding water, and let one of them have a rope faſtened to it by one of its ends, the other end being pulled by a power placed in the other boat. Here the power pulling the rope makes an equal effort to puſh from it the boat wherein it is, as to pull toward it the other, to which the rope is faſtened; ſuppoſing the direction of the power to be the ſame as that of the rope: In conſequence whereof, the two boats muſt approach equally to each other; and if the effort of the power be given, as alſo the ſurface of the boats immerged in water, the velocity of their appulſe may be determined. But in regard the ſame effort is made on both boats, to pull the one and puſh the other, the quantity of water which determines or meaſures ſuch effort, would be the ſame for the motion of each, and conſequently the heights of water, as in the caſe of reſervoirs, which would produce this effort, would be to each other, in the reciprocal ratio of the ſurfaces of the boats, preſented to the motion; but the roots of theſe heights give the velocity, and therefore the velocity of the boats would be to each other in the reciprocal ratio of the roots of the ſurfaces of the boats.

After the like manner, if the velocity of the boats, together with the ſurfaces, were given, the effort of the power that would move them might be determined; one inſtance or two will make this propoſition clear.

Suppoſe two boats in a ſtanding water, and let the ſurface of one of them be 16 feet, and that of the other 64: I mean only the ſurfaces preſented to the motion, or which are directly oppoſite one to the other. If the boat of 16 feet ſurface, either puſhed or pulled, with a force

equal to that which puſhes or pulls the other of 64 feet, the powers which puſh thoſe ſurfaces may be conſidered as 2 ſolid parallel epipeds of water, equal to each other, conſequently their heights will be reciprocally as their ſurfaces, whatever the quantity of theſe ſolids may be. But we have demonſtrated, that the velocities of the boats will always be as the roots of the heights of the ſolids of water; and conſequently the velocity of that of 16 feet ſurface, will be 8, the root of 64, and the velocity of that of 64 feet ſurface, will be 4, the root of 16.

Thus let what powers ſoever be employed to move the boats, the velocity of the boat of 64 feet ſurface, being expreſs'd by 8, the other of 16 feet ſurface, will advance with a velocity expreſs'd by 4, which is the ratio of 2 to 1— Hence,

If the boat of 64 feet ſurface move with a velocity which would carry it two feet in a ſecond, that of 16 feet ſurface will move with the velocity of 4 feet in a ſecond.

But theſe velocities being given, the effort neceſſary to make the two boats move towards a fixed point, placed between them in the line of their motion, may be determined by the rule above laid down. For ſuppoſe the boat of 16 feet to have a velocity of 2 feet, or 24 inches in a ſecond, its ſquare will be 576, which divided by 56, gives 10 $\frac{2}{7}$, the height of the water above the ſurface of the boat in lines, which gives the effort required for taking 72 pounds for a foot of water, we have 82 pound very nearly for the power required to move this boat 2 feet in a ſecond, as was ſuppoſed.

But ſince the velocity of this boat of 16 feet ſurface, is to be to the velocity of the other boat,

as two to one; the ſame *calculus* muſt be made for the other, by dividing the ſquare of one foot, or 12 inches, by 56, and multiplying the quotient, which is the height of water in lines, by the ſurface of the boat, the product gives the ſame 82 pounds, for the power required to move this boat the ſpace of 1 foot in a ſecond. The two boats therefore will be each moved with the ſame force of 82 pounds, by one ſingle power, and will approach each other 3 feet in a ſecond, one of them moving 2 feet, and the other 1 foot.

But as in the preſent example, we have only one power of 82 pounds, which being placed in a boat, pulls a rope to which another boat is faſtened, it follows, that this power will gather three feet of rope in a ſecond, whereas with the ſame power, one of the boats being ſuppos'd immoveable, we ſhould only gather the quantity of rope anſwering to the motion of the other boat; for it is not the quantity of rope we gather, that determines the effort to motion, as is eaſy to be obſerved; for that if there was nothing faſten'd to the rope, we ſhould gather it as faſt as we pleas'd, without making any effort at all; ſuppoſing the rope void of weight, as is here done.

If any doubt remain, as to what we have ſaid of the greater or leſſer quantity of rope gather'd, not having any effect towards diminiſhing or augmenting the effort, you need only attend to what befalls the weights * A, B, and F, two of which A and B, are faſten'd to the two ends of a rope, which paſſes over two pullies C and D, and ſuſtains a third pully E, to which is hung the weight F; for whether you raiſe or lower the

* Fig. 4.

weight A, the weight F will have the ſame effect on the weight B, tho' F riſes or falls.

Perhaps a clearer idea of what we have ſaid, in relation to the effort of a power pulling a boat, may be formed by conſidering it in a machine. Imagine therefore two ſurfaces * A and B, immerged perpendicularly in a ſtanding water, and conceive a little pully D faſten'd in the middle of the ſurface B, with its plane perpendicular to the plane of the ſurface B, the axis of the pully being laid horizontally on the ſame ſurface B; if now a rope be faſtened to the middle of the ſurface A, and paſs horizontally over the pully D, and afterwards ſuſtain a weight E, 'tis evident this weight E will make two efforts at the ſame time, each equal to the whole weight, one to pull the ſurface A, and the other to puſh the ſurface B; and further, theſe efforts will be the ſame, whether one of the ſurfaces be moveable or both; with this difference, that in the caſe of only one moveable ſurface, the weight will deſcend a ſpace equal to the motion of ſuch ſurface; whereas, if both be moveable, it will deſcend a ſpace equal to the motion of both together——For we here overlook that little acceleration the weight will have at the beginning of its deſcent; ſince it can only deſcend ſo far as the ſurfaces moves along the water. Beſides, in the preſent compariſon of a weight to a power, the acceleration of the weight is not concern'd, by reaſon the power to which the weight is compar'd, has no acceleration; and for the like reaſons we diſregard the friction of the pully on its axis, and the reſiſtance ariſing from the ſtiffneſs of the rope.

* Fig. 5.

Hence

Hence we learn, that whatever velocity a falling weight may have, its action depending on its weight, will have the same effect on both surfaces, and either pull or push them alike: since it cannot act on one of them to pull, without acting at the same time on the other to push it.

And the case would be the same, if a power supported on any *fulcrum*, or fixed body, were to lift perpendicularly a weight fastened thereto, for whether the power only sustained the weight, or whether it raised slowly or quickly; the *fulcrum* would have the load of this weight, besides that of the power. Supposing the *medium*, wherein the weight is raised, makes no resistance.

This sort of mechanism is quite different from that of bodies fastened to the arm of a lever, where the weight of the body and the space it is able to pass thro', being multiplied by each other, yield a quantity of motion to be considered in order to find the reciprocal effort of those bodies, and deduce the balance thereof. But here we are considering bodies in an uniform and continued motion, without any effort of percussion.

There are also different kinds of motions to be considered in bodies immerged in water: as if a surface * A be immerged perpendicularly in water, and to the middle thereof B a rope be fastened, which passing over the pully P, is turned back to D, where a power is applied. Here it is evident, that the power pulling the rope in D, and in so doing pushing the surface A, exerts the same force to push the surface towards P, as to pull it towards P, and consequently the surface A is impelled towards P, with a force double to that of the power.

* Fig. 6.

Note

Note alſo, that in this diſpoſition of machines a weight might be applied in lieu of a power, by fitting a little pulley in the ſurface A, and paſſing a rope over the ſame to which a weight is faſtened.

Hence appears the great advantage of this method of application, ſince in virtue hereof one and the ſame power placed in the boat has an effect double of what it would have in walking on the ground, and thus pulling the boat; or even of what it would have, tho' placed in the boat, but ſo as to pull a rope faſtened to a fixed point, in order to make the veſſel advance thereto. ——— Indeed this power, which is only $\frac{1}{2}$ of the other, muſt move double the ſpace of that other in the ſame time: but ſuch exceſs of the ſpace of the power is not to be conſidered as an augmentation of force, as has been already obſerved.

We ſee alſo, that this way of applying a power, by a returning pully, amounts to the ſame, as the 2 boats made to approach one another; tho' in that caſe, the power is applied to 2 ſurfaces, and in this only to one; for which reaſon, if the ſingle ſurface be leſs than the 2 others together, either a leſs power will ſuffice to give the ſame velocity, or the ſame power will ſuffice to give a greater velocity.

The following experiments confirm what we have advanced concerning the augmentation of the power, by means of returning the pully; ſeating myſelf on a ſort of ſledge, in a place paved with ſmooth ſtones, I fixed a rope on a wall 5 or 6 fathoms from me, and about the ſame height with myſelf, ſo that the rope was horizontal; in this diſpoſition I pulled the end of the rope to draw myſelf, towards the fixed point; but all my ſtrength was ſcarce able to make me advance. Upon this a pully was fixed in the place of the fixed point, over which I paſſed the rope, then

faſten

faſtening one of its ends to the ſledge, and pulling by the other, I found no difficulty in drawing myſelf along — for it is evident, from what has already been ſaid, that only half the force employed in the former caſe, was required here.—

But the great friction of the ſledge upon the pavement, induced me to make another more ſimple experiment, to the ſame purpoſe.—Faſtening a rope to a high place, I endeavoured to clamber up the ſame by ſtrength of arms: but finding myſelf too weak thus to raiſe my whole body on one arm, by holding with one hand, and taking higher hold of the rope with the other, I faſtened a pully in the place, where the rope was faſtened before; andpaſſing the rope over this pully, made a ſort of ring with one end thereof, where putting my foot, and pulling the other end of the rope, I drew myſelf on high with a deal of eaſe.

Here it is obvious, that in ſuſtaining the body by ſuch rope and pully, the arms only make half the effort which they muſt to ſuſtain the body by a rope only. For the pully in ſuch caſe, has the effect of the *fulcrum* of a balance, at the extremes of whoſe arms the whole weight of the body is ſuſtained, but ſo as to be diſtributed equally on both ſides; in which ſtate of equilibrium the effort of the arms need only be half the weight of the body; ſo that the ſmalleſt effort more, *e. gr.* a pound, will diſcharge the other half of ſo much, upon which the arms will ſurmount the weight of the body by two pounds; ſo that gathering the rope at the ſame time, the body will eaſily be hoiſted up the pulley. Hereto we overlook the friction of the axis of the pully, and the other difficulties, which may ariſe on the part of the rope.

As

As the velocity wherewith the body is raiſed, it depends on that of the arms in gathering the rope; nor need there any augmentation of power on that account, ſince the reſiſtance of the medium is not increaſed thereby. — Indeed if the experiment be made in the water, and the power be placed for inſtance in a boat, where it pulls a rope; which paſſing over a pulley, has its other end faſtened to another boat. Here, ſuppoſing the two boats equal, the power muſt be twice as great, as would be required, if the rope was faſtened to the ſame boat where the power is; by reaſon the reſiſtance of the water againſt the two boats is double of what is againſt one, which does not hold in air, the reſiſtance whereof to any moderately ſlow motion is too inconſiderable to be regarded. Beſides, that this ſame reſiſtance of the water requires an effort proportional to the velocity of the motion.

But theſe two motions may be diverſified, and conſidered in other manners. — Suppoſe a boat in a ſtanding water pulled by two oppoſite powers, applied to two ropes in the ſame right line, 'tis evident, that if the powers be equal, the boats will remain immovable. If one exceed the other, the boat will approach towards the ſame, and ſuppoſing the two powers given, the way or ſpace it will paſs over may be determined. For ſeeking by the above rule, that two velocities belonging to the 2 powers, the difference thereof will be the velocity wherewith the boat approaches the greater power, and conſequently gives the quantity of rope this power gathers, and the other loſes.

What has been ſaid of boats drawn by ropes, holds equally of boats, if the ſame be puſhed, or

ſhoved

ſhoved directly by oppoſite powers, whether they be equal or unequal.

It remains to ſhew the application of this doctrine to the operation of rowing, *i. e.* the motion of boats, *&c.* by means of oars.—'Tis not eaſy to frame a ſimple machine, which ſhall ſhew the ſurface a boat preſents to the motion of the water, and fit oars therein, whoſe *fulcrum*, or point of ſupport, called by mariners the *filling*, ſhall be in the middle of this ſurface: but it is more difficult ſtill, to apply the moving power to the end of ſuch oars, and contrive that this ſame power, *i. e.* the rowers, ſhall be ſuſtained preciſely on the middle of this ſurface. However, as the length of the boat only bears a ſmall proportion to the ſurface it oppoſes to the motion of the water; it will make no great difference, tho' the oars be reſted on the edge of the boat, inſtead of the middle of the ſurface, which is the true point of ſupport, or even tho' the power, which pulls the end of the oars, do not act in a direction perpendicular to the oar, and to the ſurface at the ſame time.—For as to the direction of the power, there can only ariſe hence a ſmall diminution of the force that acts on the one and the other, ſince it is to be conſidered withal that theſe directions are continually changing.

To bring the matter to an eaſy *calculus*, without entering into a long and intricate diſcuſſion; ſuppoſe * A the ſurface, which the boat preſents to the motion, whoſe length we have no regard to, in as much as this makes no effort againſt the motion of the water. To the middle of this ſurface is faſtened perpendicularly a *fulcrum*, or *filling* A B, upon whoſe extreme B, the oar D C

* Fig. 7.

bears, whoſe broad end or *blade*, is repreſented by the ſurface C. Laſtly the power is ſuppoſed in D, pulling perpendicularly that end of the *ſhank* of the oar, and bearing alſo perpendicularly againſt the power A of the ſurface A. Here then it is evident, that the power pulling the end D of the oar towards G, and bearing againſt the ſurface in A, will bring the blade C towards F. But the abſolute power wherewith this is done, muſt be reduced to a relative one, according to the length of the arms BD, BC, of the oar conſidered as a lever: for BC will always be to BD, as the abſolute power to the relative power applied in C, to puſh towards F. But the *fulcrum* itſelf B, or the ſurface A, is alſo impelled towards G, with a force, which is the ſum of the abſolute and relative powers added together: and, on the contrary, the ſame ſurface A, being puſhed by the abſolute power towards B or F, the equal and oppoſite efforts deſtroying each other, nothing remains of the effort wherewith the ſurface A is impelled towards G, but that of the relative power, which is the ſame as that wherewith the ſurface C is impelled towards F; ſo that theſe two efforts will be equal in all caſes.

Now to determine the velocity of the motion of thoſe ſurfaces, the power being given, or elſe the velocities and the way being given to determine the power: we ſhall make a *calculus* according to the preceding rules.

Suppoſe, for example, the ſurface A be 80 feet, and the power be 1000 pounds; *i. e.* able to ſuſtain a weight of 1000 pounds, dividing 1000 by 80, the quotient 12½ will be the weight belonging to each foot of the ſurface: and ſuppoſe 72 pounds, the weight of a foot high of water under

under a foot ſurface, *i. e.* a cubic foot of water, as 72 *lb.* are to $12\frac{1}{2}$ *lb.* ſo is 1 foot, or 144 lines height of water under a foot ſurface to 25 lines height of water, under the ſame ſurface. I ſay therefore by the firſt rule, as 2016 lines, a conſtant or given number in all caſes (or elſe as 18 lines) to 25 lines height of water; ſo is 784 a conſtant number (or elſe 7) to $9\frac{3}{4}$, whoſe ſquare root is ſomewhat above 3 feet, the velocity or way the boat will make in a ſecond of time. But now if the ſurface C be triple the ſurface A, *i. e.* be 240 feet; we ſhall find, ſuppoſing the ſame power of 1000 *lb.* acting againſt the ſame ſurface a quantity of water 4 *lb.* $\frac{1}{6}$ for each foot. Therefore as 72 *lb.* are to 46 *lb.* $\frac{1}{6}$; ſo are 144 lines height of water, under a ſurface of one foot, to 8 lines and ſomewhat more height of water; which is a third part of the height found for the other ſurface. For the power being the ſame, the heights of water muſt be reciprocally as the ſurfaces; and this ſurface is ſuppoſed triple of the other.

Now by the rule, as 2016 lines a conſtant number (or even 18) is to 8 lines $\frac{1}{3}$, ſo are 784 a conſtant number (or even 7) to $3\frac{1}{3}$ nearly; whoſe ſquare root is 1 foot $\frac{3}{4}$ nearly, which is the velocity or way the oar C will make in a ſecond, by the effect of the power.

But we have found, that the ſame power which is always the relative, with regard to the abſolute power, impells both the boat and oar, and throws them both off with regard to a fixed point, found betweeen the two; *viz.* the boat at the rate of 3 feet in a ſecond of time, and the oar 1 foot $\frac{3}{4}$ in the ſame time. It follows therefore, that the boat and oar will recede from each other the ſpace of

4 feet $\frac{3}{4}$ in a ſecond of time, being the ſum of the ways of the boat and oar: but if the velocity or way of the boat, a determinate in time, be given with regard to a fixed point, the power required thereto, will be found by the ſecond rule: ſuppoſe, *e. gr.* the way to be 30 fathoms in a minute, and that the ſurface of the boat, which it preſents to the motion, be 4 feet, the boat then will go 3 feet in a ſecond; ſo that by the ſecond rule, the ſquare of 3 feet or 36 inches, which is 1296, being divided by 56, a given number, gives $23\frac{1}{7}$, the lines in the height of water to be ſuppoſed over the ſurface of 4 feet. But an inch high of water upon a ſurface of one foot, weighs at the rate of 72 *lb.* the cubic foot; the 23 lines therefore will yield 11 *lb.* 15 ounces nearly, which multiplied by 4, the feet in the ſurface of the boat yield 47 *lb.* $\frac{3}{4}$ for the effort of the power, which would make the boat advance, as if it ſuſtained ſuch weight.

But it muſt be obſerved, that this effort is only that of the relative power; and that the abſolute power may be greater or leſs, according to the different lengths of the arm of the oar; ſo that the relative power muſt be reduced to the abſolute one, by ſaying, as the length of the arm of the oar, from the point of ſupport to the hand, is to the length of the other arm, from the ſame point of ſupport to the middle of that part of the blade of the oar which enters the water; ſo is the power found by the calculus to the abſolute power.

An application of the above rules to the motion of boats croſſing the Seine, *and other rivers.*

Here we have no regard to the current of water, but confine ourſelves to the conſideration of the

the way the boat makes directly a-croſs, tho' it be all the while driven downwards by the ſtream. And we ſuppoſe withal, that no part of the effort made by the rowers, is employed againſt the current of water. Now a waterman will row a boat about 20 fathoms or 120 feet in a minute of time, the ſurface ſuch boat preſents to the motion of the water is about 3 feet; and the ſurface of the 2 oars immerged in the water, may be about 7 feet. Laſtly, the diſtance from the point of ſupport to the rower's hand, is nearly the ſame as that from the middle of the oar immerged in the water, to the ſame point of ſupport: ſo that in this caſe the relative power is equal to the abſolute one.

By the velocity of the boat here ſuppoſed, we have 2 feet or 24 inches for each ſecond of time; and by the rule, the ſquare of theſe 24 inches is 576, which divided by 56, a ſtanding number, gives 10 lines $\frac{1}{5}$ for the height of water over the ſurface propoſed: an effort not very conſiderable, ſince a man, by means of a ſimple pully, will eaſily raiſe 15 or 16 *lb.* weight the ſpace of 2 feet in a ſecond of time. As to the motion of the hand, it muſt be twice as much with the ſame power, *viz.* 4 feet in a ſecond, for that the blade of the oar recedes from the fixed point, as far as the boat itſelf, but in a contrary direction.

We have no regard here to the interruptions in the motion of the rower; for if he only works half the time, he will be enabled to make double the effort in that half, by reſting the other; eſpecially in the preſent ſituation, for remaining in the ſame place, his arm only makes half the effort that would be required, were he to tow a boat on the edge of the water; beſide the further fatigue

tigue of walking, and ſupporting of the weight of his body.

Another application to the motion of gallies.

The ſurface a galley preſents to the motion of the water, is 80 feet, and it has 26 oars on each ſide, or 52 in all; each oar being 36 feet long, and the blade 9 inches broad. There is uſually 4 feet and $\frac{1}{2}$ in the water, and the diſtance from the *fulcrum* of the oar to the hand, is only a third of that from the *fulcrum* to the middle of the part of the blade which dips in the water: ſo that the ſurface of that part of the oar, which in the water is 3 feet $\frac{3}{8}$, and this ſurface in all the oars together 175 feet. Now a galley with 3 men to each oar, uſually makes 2800 fathoms in an hour, and conſequently 46 fathom, 4 feet, or 280 feet, in a minute: or laſtly, 4 feet, 8 inches, or 56 inches, in a ſecond.——Now it is required to find the power which the rowers exert in order hereto.

By the ſecond rule, we take the ſquare of 56 inches, *viz.* 3136, which divided by 56, a ſtanding number, gives 56 lines for the height of water over the 80 feet ſurface propoſed, conſequently multiplying 56 by 80, we have 4480 lines height of water on a foot ſurface, which divided by 144 lines for a foot height, give 31 feet, 1 inch, 4 lines, for the whole load or reſiſtance, which being reduced to weight, at the rate of 72 pound the cubic foot, yields 2240 *lb.* for the effort of the relative power of the rowers, which acts equally on the blade of the oars, and the ledge of the galley. Now dividing this weight among 156 rowers, there only will be 14 *lb.* $\frac{1}{7}$ for

for the relative power of each, reducing therefore the relative power, by saying as 1 is to 2, which is the *ratio* of that part of the oar between the *fulcrum* and the hand, to the part from the same *fulcrum* to the middle of that part plunged in the water; so is the relative power 14 *lb.* $\frac{1}{3}$ to the absolute 28 *lb.* $\frac{2}{3}$.

Farther, each rower making such effort of 28 *lb.* $\frac{2}{3}$ in a second, needed only push the extremity of the oar with his hand the space of 2 feet, 4 inches, provided the shank of the oar rested on a fixed point, by reason the way of the oar is to the way of the hand, as the distances of the *fulcrum*, which are 2 to 1. But in regard the oar does likewise recede from the same fixed point, between the galley and the oar, with regard to which the way of the hand has been found to be 2 feet, 4 inches, we must also determine, what way the shank of the oar makes in a second. It has been already found, that the power acting against the surface of a galley of 80 feet, is 2240 *lb.* and 'tis this same power which acts against all the oars, which are 175 feet $\frac{1}{2}$ in surface. The height of water therefore upon these oars, will be reciprocal of that on the surface on the galley, which we have found to be 56 lines. Consequently we may say, as 175 feet $\frac{1}{2}$ is to 80 feet, so are 56 lines to 25 lines $\frac{1}{2}$ nearly, which must be the height of water on the surface of all the oars, or each thereof; wherefore by the first rule, as 2016 lines, a given number, are to 25 lines $\frac{1}{2}$ height of water; so are 784, a given number, to 10 nearly, whose square root is 3 feet $\frac{1}{7}$, or 38 inches, which the oars must make in a second, at the same time that the body of the galley advances 56 inches.

But

But on account of the different lengths of the arms of the oar, which are as 2 to 1, the hand need only make 19 inches, or 1 foot, 7 inches in a second, with regard to the motion of the oar; and consequently the rower's hand, which makes 2 feet, 4 inches in a second, with regard to the motion of the galley, will make in all 3 feet, 11 inches in each second of time, with the same force of 28 *lb.* $\frac{2}{3}$.

This effort and motion have nothing in them extraordinary; and as in the common practice of oars, the labour is interrupted, it will be easy to make up for the time lost hereby by pulling, when the stroke returns with so much the more vigour.

'Twill now be easy to determine the power required to row a vessel up a river, and what will be the velocity thereof with any power assigned for, if both the power and the time be given, there needs only to find, as already taught, the way it must make in a standing water, and deduct from this the way or drift of the stream, the remainder will be the way the boat makes up the river in the given time; provided the motion of the boat considered as in a standing water, be greater than that of the water; for if it be less, 'tis evident the boat will drive notwithstanding the rowers.

If the way be given which a vessel is to be rowed in a given time up a river, whose motion or velocity is also given, those two motions must be joined together; and it must be found by the preceding rules, what the power is that will sustain them, acting on the surface of the boat presented to the motion of the water, which same power acting also against the oars, must be reckoned for in the motion of the arm or hand of

the rower, as was before done in gallies. For an inſtance, if a river runs 3 feet in a ſecond of time, and you would have the veſſel likewiſe aſcend 4 feet in the ſame time againſt the ſtream, here will be a velocity given of 7 feet in a ſecond, which muſt be conſidered, in order to make the calculus as in ſtanding water.

As to any machines that may be uſed to this end, we know in the general, that they are of no other advantage but to take the benefit of natural powers, as the motion of water and wind by uſing ſails, *&c.* and that by their means we can ſave time by uſing a greater power, or improve a little power, ſo as to produce a great effect. But in the motion of boats and gallies, by means of oars, it is eaſy to ſee, that no great motion can ever be produced by a weak power; for if the machine increaſe the effort, it will take up the more time, ſuppoſing the time and the way to be always in the ſame *ratio*. But the caſe is otherwiſe in animals, which pull or thruſt in walking or moving; for it is an hypotheſis of ſome mechanicks, whereby they would prove, that a man, who can lift 100 *lb.* 10 feet high in a minute, might lift a weight of 1 *lb.* 1000 feet high in the ſame time; tho' this be really impoſſible, ſince a man can only move a certain ſpace in a given time, even without any additional weight at all.

From the whole, we may learn what is to be expected from the generality of engineers, who can undertake to bring boats up any very rapid rivers, by the uſe of ſome machine, whoſe effects they are not always over well acquainted withal, imagining the force of their genius, without ſtudy, ſufficient to enable them to determine the effort neceſſary to

ſurmount their reſiſtance of fluid bodies, either in a ſtate of reſt, or moved by ſome machine, which is a mechaniſm of a higher kind, and a more difficult conſideration than that of ſolids, where the *equilibrium* is all that is uſually to be conſidered.

An

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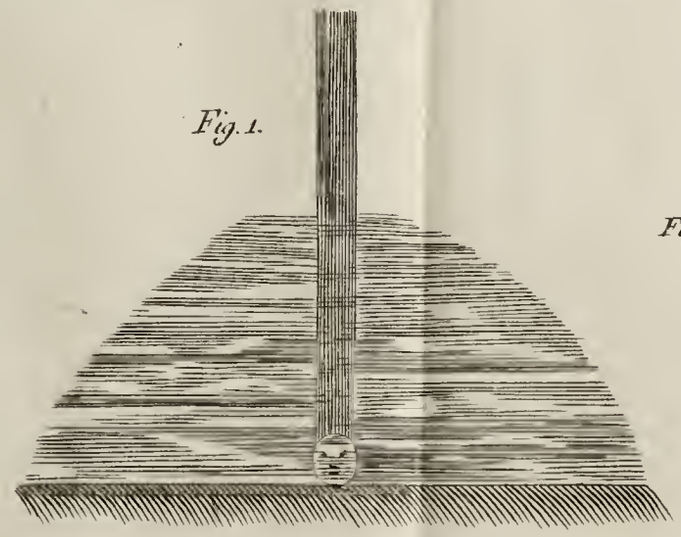
Fig. 1.



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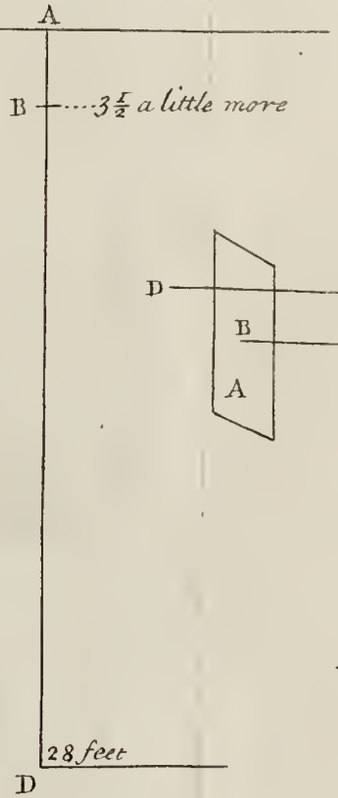
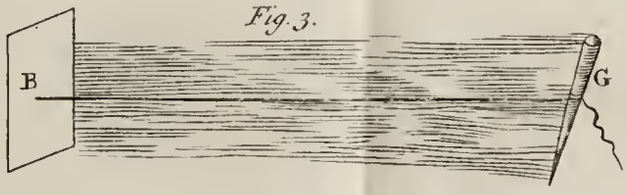


Fig. 3.



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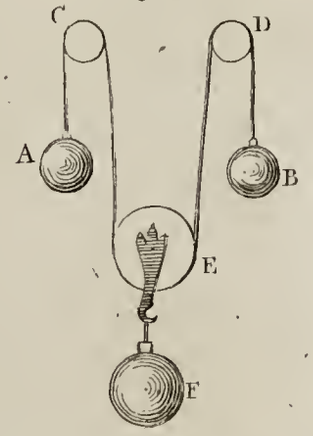

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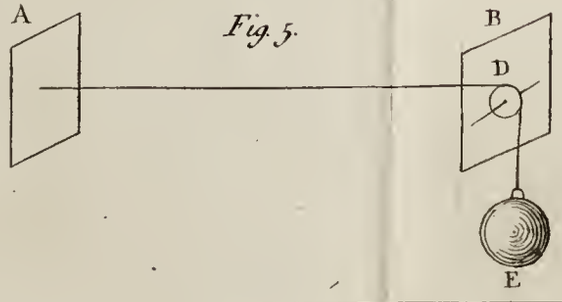


Fig. 6.

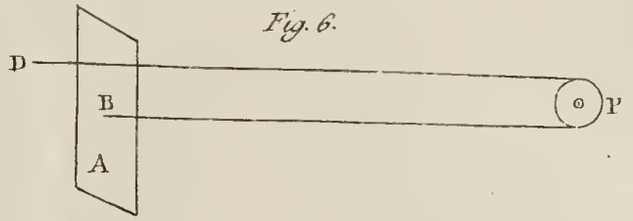
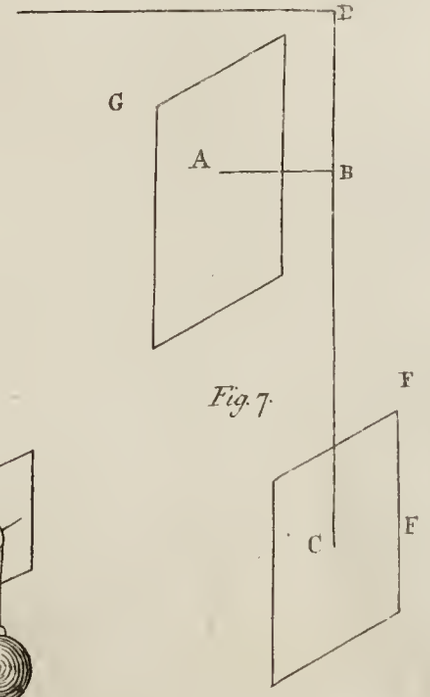


Fig. 7.



J. Mynde sc.

An EXPLANATION *of the* TERMS *of* ART *used in this* Volume.

HAVING ſet down the weights and meaſures among the terms of art, with the proportion of them to thoſe uſed in *England*, I ſhall take this opportunity of informing the reader, that all the weights and meaſures, which are mentioned in this tranſlation, are to be underſtood of thoſe uſed at *Paris*. I did not think it convenient to reduce them, in the body of the work, to the *Engliſh* ſtandard; becauſe it would have continually occaſioned the uſe of a great number of fractions, which would have made the reading troubleſome. Thoſe, who have curioſity enough to deſire an exactneſs in theſe particulars, may eaſily reduce them by the proportions here ſet down. In papers, where no very great exactneſs was required, I have tranſlated *pinte* a quart, and *chopine* a pint; but where exactneſs was required, I have preſerved the *French* words, *pinte* and *chopine*.

A

Adjutage, of a fountain, is a tube fitted to the mouth of the veſſel, through which the water is to be played, and determined to any figure.

Aorta, the great artery, from which all the reſt riſe.

Apex of the heart, the lower point which is turned a little toward the left ſide.

Apophyſis, an eminence or protuberance.

Arbor the principal part of a machine, which ſerves to ſuſtain the reſt; alſo a ſpindle or *axis* upon which a machine turns.

Aſpera arteria, the wind pipe.

Auricles, the two appendages of the heart.

Axillary veins and arteries are thoſe which paſs under the arm-pits.

B

Baſis of the heart, the upper and broader point of it.

Blade of an oar is the broad thin part, with which they ſtrike the water.

Bronchia, the little tubes, into which the *trachea* is branched.

C

Carotids, two arteries, one on each ſide of the neck, which convey the blood from the *aorta* to the brain.

Chopine, half a *Paris pinte*.

Chryſalis, called alſo *aurelia* and *nympha*, is that ſtate of an inſect between a worm and a fly, in which it lies incloſed in a huſk, as if it was dead.

Cœliac artery, the firſt artery, which is ſent from the deſcending trunk of the *aorta* into the *abdomen*. The *cœliac* vein is that which runs through the *inteſtinum rectum*.

Coronary. The *coronary veſſels* are the arteries and veins, which ſurround the heart, to nouriſh and ſupply it with blood. The *coronary arteries* carry the blood into the ſurface of the heart. The *coronary vein* returns the blood into the *vena cava*.

Cranium the ſkull.

Culm, I have made uſe of this word, to expreſs the ſtalk of any ſort of corn or graſs: it is called in *Latin culmus*.

D

Dram, the 8th part of a *Paris* ounce. It contains 72 of their grains, or 59 grains $\frac{1}{2}$ troy.

The

The dram is divided into 3 deniers; the denier into 24 grains; the grain into 24 carats.

E

Eye-glaſs of a teleſcope is the glaſs placed at that end of the tube, which is neareſt the eye.

F

Foot, a meaſure containing 12 inches, or 144 lines. The *Paris* foot, or *pied de roy*, is longer than the *Engliſh* foot, which contains but 11 inches, 4 lines $\frac{1}{2}$. The *Rhinland*, *Rheniſh*, or *Leyden* foot is to the *Paris* foot, as 1390 to 1440. The ancient geographical foot and the modern *Roman* foot, are to the *Paris* foot, as 11 to 12. The *Bolonian* foot is to the *Paris* foot, as 701 to 600.

Foramen ovale, an oval aperture, or paſſage through the heart of a *fœtus*, which cloſes up ſometime after the birth.

Fulcrum, the prop or point of ſupport of a lever.

G

Ginglymus is that kind of articulation, where each bone mutually receives the other. There are three ſorts of it: 1. When the bones receive each other reciprocally, after the manner of a hinge, as at the elbow. 2. When one bone receives another at one of its extremities, and is received into another at the other extremity, as the *vertebræ* do. 3. When one bone is received into another, after the manner of a wheel, or axle of a wheel in a box; as the ſecond *vertebra* of the neck in the firſt.

I

Inch, the twelfth part of a foot. It contains $\frac{82}{1000}$ parts of an *Engliſh* foot, and therefore is

a little more than 6 ſuch parts longer than the *Engliſh* inch.

L

Lamella, a little thin *lamina*, or plate, ſuch as a ſcale of a fiſh; it is often uſed for a ſubdiviſion of *lamina*.

Lamina, a thin plate.

Line, the twelfth part of an inch.

Lunulæ, little creſcents, or half-moons.

M

Maxillæ, the jaws.

Mediaſtinum, a double membrane, which divides the *thorax* and lungs into two parts.

Meſenteric; the arteries are ſo called, which ariſe from the *aorta*, and proceed to the meſentery; and the veins, which proceed from the meſentery.

O

Object-glaſs of a teleſcope, is the glaſs placed at that end of the tube, which is neareſt to the object.

Ounce, the ſixteenth part of a pound. It contains 576 *Paris* grains, or 476 grains troy. It is equal to 19 penny-weight, and 20 grains troy.

P

Pericardium, the membrane, which incloſes the heart.

Peritonæum, a thin ſoft membrane, which covers and contains all the bowels in the lower belly.

Pinte, the *Paris pinte* contains two of their pounds of water, that is, 15,232 grains troy, or 2 pounds, 2 ounces, and 13 grains averdupois; therefore the *Paris pinte* is larger than our quart, which is ſuppoſed to contain 2 pounds averdupois of water.

Piſton

Piſton the ſucker of a pump, ſyringe, or ſuch like machine.

Pivot, a foot of metal, whereby a body intended to turn round bears upon another which is fixed.

Pound; the *Paris* pound contains 16 of their ounces, or 9216 grains. It is equal to 15 ounces, 17 penny weight and 8 grains troy.

R

Retina, the innermoſt coat of the eye, ſo called becauſe it is woven like a net.

Rundle, that part of a pulley, which is like a wheel.

S

Sigmoidal valves, ſo called from their reſembling the *Greek* letter *ſigma*, which was anciently written like our C.

T

Thorax, the capacity of the breaſt, or cheſt, containing the heart and lungs.

Toiſe, a meaſure containing 6 *Paris* feet, or 6 *Engliſh* feet, and $\frac{4}{10}\frac{8}{00}$ parts. Mr. *Chambers* tranſlates it *fathom*, which is a meaſure of 6 *Engliſh* feet.

Trachea, the wind-pipe.

V

Venæ cavæ, the veins which carry the blood into the heart.

Vena porta, the vein which carries the blood into the liver.

Ventricles of the heart, the two great cavities of it; the tortoiſe is ſaid to have three.

Vertebræ, the bones of the hinder part of the neck, and of the back, commonly called the ſpine or chine.

Z

Zymoſimeter, an inſtrument propoſed by *Swammerdam*, to meaſure the degree of fermentation cauſed by the mixture of ſubſtances, and what heat they acquire by fermentation, and the degree of heat or temperament of the blood of animals.

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London: Printed for *John Nourſe*, at the *Lamb* without *Temple-Bar*.